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PSYCHOLOGY AND PHILOSOPHY

I.—Mr. STRAWSON ON LOGICAL THEORY

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A PHILOSOPHER of ordinary language has brought his limpid vernacular to bear on formal logic.[1] Step by unhurried step he explains the terms of logical appraisal and what the logician's business is, and sets the logician's artifacts over against the speech of natural man. '⊃' emerges at page 34, the truth tables at page 68, the quantifiers at page 131, and the syllogism at page 158. The intervening and ensuing space is given over not to theorems and proofs and decision procedures (except for some sketchy examples), but to interpretation and criticism. A ninth chapter, the last, is an excellent little philosophical essay on induction.

The division of the present review into sections will correspond to the structure not of the book, but of the critical reflexions which the book has stimulated in this reader.

1. *Entailment, analyticity, and company*

First Mr. Strawson undertakes to explain, in an ordinary-language setting, the notions of inconsistency and entailment. The devices at his disposal are analogy and example; and even the method of example offers difficulties, since he is at pains to withhold the stigma of inconsistency from speech fads like "Well, I do and I don't".

Engineers have been known to work wonders with the differential calculus without ever having been given an account of its foundations more intelligible than the notion of an actual

[1] P. F. Strawson, *Introduction to Logical Theory*. 1952. Pp. x + 266. London: Methuen & Co. New York: John Wiley & Sons.

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infinitesimal; and there are philosophers who have, through use and custom, grown to feel equally at home with the notion of entailment which so pervades G. E. Moore's philosophical analyses. There are philosophers of ordinary language who have grown so inured to the philosophical terms 'entails' and 'inconsistent' as to look upon them, perhaps, as ordinary language. But the reader without such benefits of use and custom is apt to feel, even after Mr. Strawson's painstaking discussion of the notions of inconsistency and entailment, somewhat the kind of insecurity over these notions that many engineers must have felt, when callow, over derivatives and differentials. At the risk of seeming unteachable, I go on record as one such reader.

Turning away from Mr. Strawson's book for a bit, let us seek perspective on the general problem. The terms 'entail' and 'inconsistent' belong to a group other members of which are 'analytic' and 'synonymous'. Because of the easy interdefinability of these terms, one of them suffices to represent the group; and a handy choice is 'analytic'. In recent classical philosophy the usual gesture toward explaining 'analytic' amounts to something like this: a statement is analytic if it is true by virtue solely of meanings of words and independently of matters of fact. It can be objected, in a somewhat formalistic and unsympathetic spirit, that the boundary which this definition draws is vague or that the definiens is as much in need of clarification as the definiendum. This is an easy level of polemic in philosophy, and no serious philosophical effort is proof against it. But misgivings over the notion of analyticity are warranted also at a deeper level, where a sincere attempt has been made to guess the unspoken *Weltanschauung* from which the motivation and plausibility of a division of statements into analytic and synthetic arise. My guess is that that *Weltanschauung* is a more or less attenuated holdover of phenomenalistic reductionism.

A philosopher may have rejected phenomenalism in the full reductionistic sense, in favour of admitting that statements for the most part are laden with an irreducibly extra-phenomenal burden over and above their phenomenal import. But he may continue to hold (*a*) that the statements do still possess their phenomenal import, what there is of it, as separate statements one by one; or he may hold rather (*b*) that the statements are tied to the testimony of the senses only in a systematic or holistic way which defies any statement-by-statement distribution of sensory certificates. If he holds so much as the

vestige (*a*) of phenomenalistic reductionism, then he will find it natural to accept in principle a division between analytic and synthetic truths, the former being those in which the phenomenal content is null. If on the other hand his position is (*b*), he may be expected to find no way of putting some truths into empirical quarantine and judging the remainder free of infection. For him the contribution which linguistic meaning makes to knowledge and the contribution which sensory evidence makes to knowledge are too inextricably intertwined to admit of a sentence-by-sentence separation.

My own position is (*b*). I do grant that any given sensory event seems more relevant to some statements than to others; also that some statements seem less directly touched than others by sensory events in general; but I think these variations can be accounted for as sporadic surface effects, without prejudice to (*b*) as underlying principle.[1] My misgivings over the notion of analyticity are thus misgivings in principle. But those also who espouse the notion espouse it mainly in principle, granting freely that the boundary between the analytic and synthetic can be troublesome and indecisive in application.

The purpose of the foregoing excursion is not to invoke my philosophy in criticism of another man's book. There are rather three other points. One is that misgivings over analyticity and related notions are not just a cavilling over fuzzy boundaries. A second is that these notions are too bound up with a debatable philosophical position to be well suited to the very prominent roles which Mr. Strawson assigns to them in his project of clarifying elementary logic from the standpoint of ordinary language. A third is that I should think Mr. Strawson himself, with his stress on the realities of common-sense language, would incline rather to (*b*) than to (*a*).

He frequently shows awareness that there are difficulties in applying analyticity and related notions. On page 5 he writes:

> What makes predicates incompatible? . . . It is we, the makers of a language, who make predicates incompatible. . . . A boundary must be drawn, limiting the applicability of a word used in describing things; and it is we who decide where the boundaries are to be drawn.
>
> This metaphor of drawing boundaries is in some ways misleading. I do not mean by it that we often make conscious decisions of this kind . . . nor that the decisions we make when we make them, are purely verbal decisions.

[1] See my "Two dogmas of empiricism", *Philosophical Review*, vol. lx (1951), pp. 20-43, especially pp. 40 ff.

On page 91 he writes :

> What we are suffering from here is perhaps a crudity in our notion of entailment.

On page 231 he writes :

> We may very often hesitate to say whether a given sentence is analytic or synthetic ; and the imprecision of this distinction, as applied to ordinary speech, reflects an imprecision in the application of the notion of entailment to ordinary speech.

The laudable observation last quoted might well have caused a philosopher of ordinary language to wonder about his use of the notion of analyticity as a keystone. But, as I have urged in earlier pages, the notion has yet a more serious fault than vagueness.

2. *Logical truth*

If the author has chosen too soft and friable a keystone in analyticity, then it is fair to ask what he could have used in its place. Insofar as he uses the notion of analyticity in defining the province of logic, my answer is as follows : he could have used, instead, the notion of truth and the notion of logical vocabulary. Given these, the business of formal logic is describable as that of finding statement forms which are *logical*, in the sense of containing no constants beyond the logical vocabulary, and (extensionally) *valid*, in the sense that all statements exemplifying the form in question are true. Statements exemplifying such forms may be called *logically true*. Here there is no hint of a doctrine as to the epistemological grounds of logical truth ; no affirmation or denial of conventionalism (whatever that would be), nor any effort to separate the analytic from the synthetic.

Mr. Strawson observes (pp. 40 ff.), and rightly (if for the moment we set aside any misgivings over the notion of analyticity), that not all analytic statements are instances of logical forms all of whose instances are analytic. As examples to the contrary he cites statements of the non-logical form :

$$x \text{ is a younger son} \supset x \text{ is a brother.}^{1}$$

However, he recognizes that instances of the latter kind are not supposed to be provided for by rules of logic. The forms which the logician wants as theorems are, by Mr. Strawson's own account, just those logical forms all of whose instances are

[1] For further discussion of this contrast see my " Two dogmas ", cited earlier.

analytic. This account matches that in my preceding paragraph, except that it has 'analytic' instead of 'true'. In net resultant scope these two accounts of logic differ little if any. They may differ in that certain logical forms whose validity depends on the size of the universe would qualify as theorems for my logic book and not for his; but I should need to understand analyticity better to be sure even of this.

I have urged, above, that Mr. Strawson's characterization of the scope of logic in terms of the notion of analyticity be dropped in favour of a characterization in terms of the notions of logical vocabulary and truth. Logical vocabulary is specified only, I suppose, by enumeration. If this element of apparent arbitrariness is a shortcoming, it is a shortcoming also in Mr. Strawson's characterization; for this also depends on the notion of logical vocabulary, via logical form. He may still feel that he brings out the essential nature of logic more fully than my characterization would do, in that the logical truths turn out for him to be some (though not all) of the analytic statements. However, one who rejects the notion of analyticity is less averse than others to finding that the boundaries of logic, like those of biochemistry and other disciplines, are to some degree capricious.

The notion of analyticity, as used in Mr. Strawson's characterization of logic, gave way to the notion of truth in my alternative characterization. The notion of truth is also of course one of which Mr. Strawson avails himself frequently in the course of his book. Possibly he considers the notion intelligible only as a sum of analyticity and empirical truth; if so, reluctance to do without the notion of analyticity is the more understandable. But in fact the inclusive notion of truth is a far less dubious starting point than that of analyticity; for we understand under what circumstances to say of *any* given statement that it is true, just as clearly as we understand the statement itself.

The group of notions to which that of analyticity gives rise, *viz.* entailment, inconsistency, and synonymy, are paralleled by a group of notions issuing from logical truth in the sense defined a few paragraphs back. Thus, just as one statement entails another, if the corresponding conditional ('$\supset$') is analytic, so for me one statement (logically) implies another if that conditional is logically true. Just as a statement is inconsistent for Mr. Strawson if its negation is analytic, so for me it is inconsistent, or logically false, if its negation is logically true. It is noteworthy, for my strictures against the notion of analyticity, that much of Mr. Strawson's use of analyticity and entailment in

the course of his logical expositions strongly resembles my use of logical truth and implication in *Methods of Logic*. On the other hand there also are long and inconclusive passages in Mr. Strawson's book which would drop out if the recommended shift were made.

3. *Words into symbols. Truth-value gaps*

Logic, under either of the accounts contrasted above, is *formal* logic in a narrow sense which excludes those preparatory operations, in applied logic, whereby sentences of ordinary language are fitted to logical forms by interpretation and paraphrase. Mr. Strawson stresses the magnitude of these applicational manoeuvres, and in this I am in full agreement. The considerations involved in them deserve attention in logic texts, and have been given attention in some; rarely, however, with the sympathetic care and subtlety which Mr. Strawson bestows on them.

One conspicuous divergence between language as used and language as depicted in logical forms is the correspondence of many idioms on the one hand to few on the other. Reduction of the rich variety of more or less interchangeable grammatical constructions and logical locutions of ordinary language to a conveniently standardized minimum is imperative for algorithmic purposes; for the power and simplicity of an algorithm, or indeed of any theory, depend on there being many occurrences of few elements rather than few occurrences of many.

Further divergence between language as used and language as reflected in logical forms remains after reductions of the kind just alluded to have been completed; for, the surviving logical particles have uses in ordinary language which diverge from the laws formulated by logicians. The well-known failure of the ordinary statement operators 'or', 'if-then', 'and', and 'not' to conform in all cases to the precepts of truth-functional logic is well expounded by Mr. Strawson. Because 'and' and 'not' deviate less radically than the others, I have found it pedagogically helpful (in *Elementary Logic*) to treat the translation of ordinary language into logical form, at the truth-functional level, as funneled through 'and' and 'not'; and Mr. Strawson follows suit.

Such failures of correspondence are not, of course, confined to the truth-function level. They recur with 'every' and 'some', in relation to the logic of quantification. Mr. Strawson also develops these details with much sensitivity.

Another conspicuous way in which ordinary language diverges from language as reflected in logical forms is in the existence of what I have called truth-value gaps. One illustration in my *Methods of Logic* is the conditional, under ordinary usage in the indicative mood. Ordinarily the conditional is not thought of as true or false at all, but rather the consequent is thought of as conditionally true or false given the antecedent. Another example, *op. cit.*, is provided by the singular description ; if the object which it purports to describe does not exist, then commonly the contexts of the description are accorded no truth values under ordinary usage. " The question of their truth ", as Mr. Strawson phrases it in his able exposition of the topic, " does not arise."

Mr. Strawson exploits this idea in a detailed defence of the traditional syllogistic logic apropos of the famous question, raised by Leibniz and others, of existential import. Mr. Strawson's method is to construe the categorical forms, for purposes of traditional logic, in such a way that where a term is empty of extension the question of the truth of the containing categorical statement does not arise. He argues plausibly that this view does considerable justice to ordinary language. His is, I expect, the best way of defending the traditional syllogistic.

A substantial offshoot of Mr. Strawson's reflexions on truth-value gaps is a theory, expounded earlier in an article by Strawson [1] and reminiscent also of Aquinas and Geach,[2] in which a distinction is made between the referential and the predicative role of a singular term. This distinction, little heeded in logical literature, is important for an appreciation of ordinary language ; and, as Mr. Strawson well brings out, it reveals a marked failure on the part of Russell's theory of descriptions to correspond to the ordinary use of ' the '.

Normally, if the role of a singular term in a given statement is referential, the question of the truth of the statement does not arise in case the purported object of the term is found not to exist. Since modern formal logic closes all such truth-value gaps, it is not to be wondered that there is nothing in modern logic to correspond to the referential role, in Mr. Strawson's sense, of terms. Mr. Strawson is at pains to point out that proper names, so-called by formal logicians, are therefore far from corresponding to the singular terms of ordinary language.

[1] P. F. Strawson, " On referring ", MIND, vol. lix (1950), pp. 320-344.
[2] P. T. Geach, " Subject and predicate ", *ibid.* pp. 461-482.

On this point he thinks to find modern logic in a difficulty. For, he writes (p. 216):

> Now the whole structure of quantificational logic, with its apparatus of individual variables, seems, or has seemed to most of its exponents, to require, for its application to ordinary speech to be possible at all, that there should exist individual referring expressions that could appear as values of the individual variables.

It is therefore important to emphasize, in contrariety to what the above quotation suggests, that anything even remotely analogous to proper names or singular terms is systematically eliminable from modern logic altogether, both in theory and in application.[1]

Granted the value of the distinction between referential and predicative roles as a means of capturing the genius of ordinary language, it would be a mistake to infer that modern logic errs in not keeping the idiosyncrasy of ordinary language which that distinction brings out. We shall recur to the general question of the function of formal logic in § 5, below. Meanwhile let us rest with this analogy: Weierstrass did not define the infinitesimal, but showed rather how to get on without it.

4. *Instability of truth value. Tense*

Another important respect in which language as used diverges from language as reflected in logical forms is the variation of truth value from occurrence to occurrence of a single sentence. Such variation can result from the use of indices ('I', 'here', 'now') or tensed verbs; also it can result from casual ambiguities, variously resolved by varying contexts and situations. Formal logic, on the other hand, developing arguments as it does in which a schematic letter 'p' keeps recurring, is misapplied unless the sentence represented by 'p' is thought of as keeping a fixed truth value at all points in the argument.

In describing these matters, Mr. Strawson adopts a double terminology: 'sentence' versus 'statement'. One and the same sentence can be used in ordinary language to make any of various statements, whereas a sentence to which formal logic is applied must be thought of as making one fixed statement and no other. In appealing thus to "statements", not as a kind of sentence but as acts performed by uttering sentences, or perhaps contents conveyed by sentences, Mr. Strawson gains a certain expository ease and also runs a certain risk. The risk is that of hypostatizing obscure entities, akin perhaps to "propositions"

[1] *Cf. Methods of Logic*, pp. 215-224.

or "meanings" or "facts" or "states of affairs", and reading into them an explanatory value which is not there.

Terminological questions aside, the variation of truth values in ordinary language and the insistence on fixed truth values for purposes of formal logic are points which are no less important than they are familiar. Mr. Strawson overstates the consequent limitation on the uses of formal logic, however, when he writes (p. 223):

> (1) that when we inquire what use can be made of the symbolic apparatus of logic we find that for certain general reasons it seems best adapted to the role of systematically exhibiting the logical relationships between sentences which answer to the ideal of independence of contextual conditions;
> (2) that the actually occurring sentences of this type are analytic sentences and law-sentences.

Formal logic would be a pretty idle luxury if its applicability were limited thus severely. In explanation of why formal logic is not really thus limited, let me quote myself (*Methods of Logic*, p. 43):

> Insofar as the interpretation of ambiguous expressions depends on circumstances of the argument as a whole—speaker, hearer, scene, date, and underlying problem and purpose—the fallacy of equivocation is not to be feared; for, those background circumstances may be expected to influence the interpretation of an ambiguous expression uniformly wherever the expression recurs in the course of the argument. . . .
>
> The fallacy of equivocation arises rather when the interpretation of the ambiguous expression is influenced in varying ways by immediate contexts, . . . so that the expression undergoes changes of meaning within the limits of the argument. In such cases we have to rephrase . . . to the extent of resolving such part of the ambiguity as might, if left standing, end up by being resolved in different ways by different immediate contexts within the proposed logical argument.

Mr. Strawson's dim view of the scope of applicability of formal logic is perhaps attributable in part to the fact that he gets into trouble over tensed and tenseless verbs on pages 150 f.:

> For example, we might try writing the sentence 'There was at least one woman among the survivors' in the form '$(\exists x)$ (x is a woman . x was among the survivors)'. But to say 'There *is* at least one person who is a woman and was among the survivors' is at least to suggest that the person is alive at the time the sentence is uttered . . . Changing the second 'is' to 'was' will not help; it will merely prompt the question 'What became of her then? Has she changed her sex?' Nor can the difficulty be evaded by declaring '$(\exists x)$' in this sentence to be timeless; it is not true that when we speak of persons and incidents the question of time-reference does not arise.

Mr. Strawson's error occurs where he says "Nor can the difficulty be evaded . . .". The only tenable attitude toward quantifiers and other notations of modern logic is to construe them always, in all contexts, as timeless. This does not mean that the values of 'x' may not themselves be thing-events, four-dimensional beings in space-time; it means only that date is to be treated on a par with location, colour, specific gravity, etc.—hence not as a qualification on '$\exists$', but merely as one of sundry attributes of the thing-events which are values of 'x'. When 'x' ranges rather over numbers, Mr. Strawson appreciates that '$(\exists x)$' is best read 'There [is] in the number series a number x such that', with tenseless '[is]'; but he does not appreciate that '$(\exists x)$' is likewise to be read 'There [is] in space-time a thing-event x such that' when 'x' ranges over the four-dimensional denizens of the ages and galaxies of space-time. Any value of 'x' in this latter or spatio-temporal universe of discourse will in fact have a time, just as any value of 'x' in the former or numerical universe of discourse will in fact have a highest prime factor; but the '[is]' or '$\exists$' itself speaks no more of time than of prime factors.

The way to render Mr. Strawson's example is '$(\exists x)(x$ [is] a woman $. x$ was among the survivors)', with tenseless '[is]' and, as always, tenseless '$\exists$'. The 'was' here involves reference presumably to some time or occasion implicit in the missing context; if we suppose it given by some constant 'D' (*e.g.* 'The sinking of the Lusitania'), then the whole amounts to '$(\exists x)(x$ [is] a woman $. x$ [is] among the survivors of $D)$', tenseless throughout.

The above example is not odd, but typical. The four-dimensional view of space-time is part and parcel of the use of modern formal logic, and in particular the use of quantification theory, in application to temporal affairs. It may be felt to be a criticism of modern logic that it calls for so drastic a departure from the time-slanted Indo-European language structure. But the better way of looking at the matter is to recognize both in the four-dimensional approach, with its notable technical advantages, and in quantification theory, with its notable technical advantages, two interrelated contributions to scientific method.

It would be hard to exaggerate the importance of recognizing the tenselessness of quantification over temporal entities. The precept has been followed as a matter of course by anyone who has been serious about applying modern logic to temporal

entities.[1] I see no reason to expect a coherent application of quantification theory to temporal matters on any other basis.

Earlier I suggested that Mr. Strawson's failure to appreciate the tenselessness of quantification over temporal entities might be a factor in his underestimation of the scope of modern logic. I should like to go further and say that I do not see how, failing to appreciate the tenselessness of quantification over temporal entities, one could reasonably take modern logic very seriously. From having perhaps wondered at Mr. Strawson's doubts over logic, one comes to wonder rather at his forebearance.

5. *The place of formal logic*

Reduction of ordinary language to logical form is, as noted in § 3 above, a reduction in at least two ways: reduction of the variety of idioms and grammatical constructions, and reduction of each surviving idiom to one fixed and convenient interpretation. That fixed interpretation is bound to be, moreover, a pretty Pickwickian one, as is evident from §§ 3-4 above. Now Mr. Strawson represents this Procrustean activity somewhat as a hobby:

> Logicians like to present a tidy system of interconnected rules. The neatness of a system might suffer if it had too many constants in it (p. 49).
>
> And it is this ideal of systematization which has most profoundly influenced the modern development of logic; so profoundly that the original conception of simply codifying the most general principles we appeal to in making our logical appraisals has pretty well been lost sight of. . . . The formal logician, in relation to ordinary language, might be compared with a man ostensibly mapping a piece of country of which the main contours are highly irregular and shifting. But the man is passionately addicted to geometry. . . . Naturally his maps will never quite fit (pp. 57 f.).

The pleasures of science are not to be denied, but the tendency to equate those pleasures with the pleasures of games can be seriously misleading. There are those, certainly, who have approached mathematics and logic in the same spirit in which they approach chess; but my suspicion, undissipated still by the fashionable tendency to cite quaternions as a case to the contrary, is that those playful spirits have been less productive of important results than those whose pleasure in science is the pleasure of working toward fundamentals. There is no deciding whether ibn-Tahir and al-Khwarizmi devised Arabic numeration

[1] Examples: Carnap, *Der logische Aufbau der Welt*; Woodger, *The Axiomatic Method in Biology*; Woodger, *Biology and Language*.

and algebraic notation in a gaming spirit, but at any rate the motivation of the Procrustean treatment of ordinary language at the hands of logicians has been rather that of achieving theoretical insights comparable to those which Arabic numeration and algebra made possible. That their hope has not been forlorn is attested by such discoveries as Gödel's of the impossibility of a complete system of number theory, and Church's of the impossibility of a decision procedure for quantification theory.

Nor need one set one's sights so high; even the humdrum spinning out of elementary logical principles in modern logic brings insights, concerning the general relation of premiss to conclusion in actual science and common sense, which are denied to men who scruple to disturb a particle of natural language in its full philological concreteness. The naturalist who observes nature only with his hands clasped behind him may gain poetic inspiration, and he may even contribute a little something to taxonomy; but he is not to be looked to for a basic contribution to scientific theory.

The ancillary activity of analyzing and paraphrasing scientific sentences of ordinary language, so as to abstract out their logical form and explore the formal consequences, is comparable in principle to the activity of the physicist who re-works and re-thinks his data and hypotheses into a stereotyped mathematical form so as to be able to bring the techniques of tensor analysis or differential equations to bear upon them. It is an important activity, and deserving of all the space and acumen which Mr. Strawson expends upon it. My only quarrel is with the notion, hinted now and again, that it is somehow wrong to have to undertake this activity, and that formal logicians have been generally seduced by hobbyism into making mistakes about language—as if Frenchmen betrayed ignorance of French when they depart from the pattern of 'soixante-dix-neuf' and 'quatre-vingts' by writing '79' and '80'.

The long and perceptive passages in which Mr. Strawson traces out something like a logic of ordinary language have all the interest and value of an able philological inquiry. But it is a mistake to think of Mr. Strawson as doing here, realistically, a job which the dream-beset formal logician had been trying to do in his unrealistic way. Actually the formal logician's job is very different, and may be schematized as follows. To begin with let us picture formal logic as one phase of the activity of a hypothetical individual who is also physicist, mathematician, *et al.* Now this overdrawn individual is interested in

ordinary language, let us suppose, only as a means of getting on with physics, mathematics, and the rest of science; and he is happy to depart from ordinary language whenever he finds a more convenient device of extraordinary language which is equally adequate to his need of the moment in formulating and developing his physics, mathematics, or the like. He drops 'if-then' in favour of '⊃' without ever entertaining the mistaken idea that they are synonymous; he makes the change only because he finds that the purposes for which he had been *needing* 'if-then', in connexion with his particular scientific work, happen to be satisfactorily manageable also by a somewhat different use of '⊃' and other devices. He makes this and other shifts with a view to streamlining his scientific work, maximizing his algorithmic facility, and maximizing his understanding of what he is doing. He does not care how inadequate his logical notation is as a reflexion of the vernacular, as long as it can be made to serve all the particular needs for which he, in his scientific programme, would have otherwise to depend on that part of the vernacular. He does not even need to paraphrase the vernacular into his logical notation, for he has learned to think directly in his logical notation, or even (which is the beauty of the thing) to let it think for him.

Not that this logical language is independent of ordinary language. It has its roots in ordinary language, and these roots are not to be severed. Everyone, even to our hypothetical logician-scientist and his pupils' pupils, grows up in ordinary language, and can learn the logician-scientist's technical jargon, from '⊃' to '$\frac{dy}{dx}$' to 'neutrino', only by learning how, in principle at least, to paraphrase it into ordinary language. But for this purpose no extensive analysis of the logic of ordinary language is required. It is enough that we show how to reduce the logical notations to a few primitive notations (say '∼', '.', 'ϵ', and universal quantification) and then explain just *these* in ordinary language, availing ourselves of ample paraphrases and scholia as needed for precision. These explanations would be such as to exclude, explicitly, any unwanted vagaries of the 'not', 'and', 'is', and 'every' of ordinary language; such also as to provide for the tenselessness, the eternal invariance of truth-value, which classical logical theory presupposes in the statements to which it is applied (*cf.* § 4 above).

Let it not be inferred from the above account that formal logic is a scientific tool without philosophical relevance; nor let it be supposed that its philosophical relevance must consist in

a point-by-point application to the recorded speech behaviour of the man in the street. Philosophy is in large part concerned with the theoretical, non-genetic underpinnings of scientific theory; with what science could get along with, could be reconstructed by means of, as distinct from what science has historically made use of. If certain problems of ontology, say, or modality, or causality, or contrary-to-fact conditionals, which arise in ordinary language, turn out not to arise in science as reconstituted with the help of formal logic, then those philosophical problems have in an important sense been solved: they have been shown not to be implicated in any necessary foundation of science. Such solutions are good to just the extent that (*a*) philosophy of science is philosophy enough and (*b*) the refashioned logical underpinnings of science do not engender new philosophical problems of their own.

One example of such elimination of philosophical perplexities is Frege's "definition" of number. Another is the avoidance, by means of quantification theory, of the misleading substantive 'nothing'. Another is the recourse to '⊃' and quantification to avoid the vernacular 'if-then', with the problems of cause and modality to which it gives rise. And the classic case is Russell's theory of descriptions. Mr. Strawson (to get back to him after an absence of a page and a half) ably shows the failure of Russell's theory of descriptions as an analysis of the vernacular usage of the singular 'the', but he shows no appreciation of the value of Russell's theory as a means of getting on in science without use of any real equivalent of the vernacular 'the'. Russell's '$(\imath x)$' is to the vernacular 'the x such that' as '⊃' is to the vernacular 'if-then'; in neither case do we have a *translation*, but in both cases we have an important means of *avoidance* for scientific purposes. And in both cases we therefore have solutions of philosophical problems, in one important sense of this phrase.

6. *Perplexity over transitivity*

Mr. Strawson compares (pp. 40-46) the forms of inference:

(1) all f's are g and x is an f $\therefore$ x is a g,
(2) x is a younger son $\therefore$ x has a brother,
(3) xRy and yRz $\therefore$ xRz.

He observes that all inferences of forms (1) and (2) are valid (indeed analytically so), whereas only some inferences of the form (3) are valid. In particular those inferences of the form

(3) are valid (analytically so) which have, *e.g.* the more special forms :

(4) x is congruent with y and y is congruent with z
$\therefore$ x is congruent with z,
(5) x is an ancestor of y and y is an ancestor of z
$\therefore$ x is an ancestor of z,
(6) x is faster than y and y is faster than z
$\therefore$ x is faster than z.

He observes further that the forms (1) and (3) are logical (*cf.* § 2, above), while the forms (2), (4), (5), and (6) are not. So far all is in order. But then he continues his discussion with a perplexing air of perplexity over (3). "Some logicians", he writes, "have felt that all those words which, substituted for 'R' [in (3)], would yield valid inference patterns *ought* to have some common verbal feature". He goes on to urge, rightly, that those logicians (whoever they may be) are mistaken. But he recurs to (3) in extended passages later in the book (pp. 53-55, 203, 207-208, 210) ; and the reader gets a sense of there still being a puzzle in the author's mind, both from the disproportionate use of space and from two particular subsequent passages.

In one of these passages (pp. 207 f.) he cites "transitively relational inference" as an example of what the traditional formal logic could not do. "Attempts", he continues, ". . . to maintain the reducibility of, *e.g.* transitively relational inferences to syllogistic form have a certain interest . . . The cruder kind of attempt merely introduces the principle . . . as a further premise, to be added to those of the original inference." But what more than this can modern logic do for "transitively relational inference" ? (3) is not a law of any logic, as the author himself has stressed in other pages. *Any* logic will need to bolster (3) with an appropriate further premiss of the form :

(7) $xRy \,.\, yRz \,.\supset xRz$,

except in those special examples whose transitivity happens to be logically demonstrable. This remark is indeed a flat tautology.

In the other passage (p. 204) he classifies as transitive those relational predicates which, if substituted in (7), yield *analytic* formulae. But standard usage requires only, for "transitivity", that (7) come out *true* for all x, y, and z. This discrepancy may suggest a clue to the author's very special concern over transitivity : is it traceable to a notion that whenever (7) holds for all x, y, and z it holds analytically ? And if he thinks

this, does he think it because (7) is a logical formula ? But this would be a mistake. Transitivity is indeed a logical trait, in that (7) is a logical formula. Likewise nullity is a logical trait, in that '$(x) \sim fx$' is a logical formula ; but the possession of nullity (or fulfilment of '$(x) \sim fx$') by the predicate of grifinhood is a matter of empirical zoology. The fulfilment of (7), for all x, y, and z, by a given predicate can be equally accidental. Example : Take 'xRy' in (7) as 'x and y are residents of the western Azores and live within ten miles of each other'. (Here the relevant facts are that the western Azores are eleven miles apart and the longer of them is ten miles long.)

For the mistaken ideas which I have attributed to Mr. Strawson in the psychological speculations of the foregoing paragraph, he is not responsible beyond having led me to speculate. There are passages, *e.g.* in the lower part of page 54 and the next page, where his views on transitivity seem quite in order ; yet the extensive further passages make one wonder. Actually the matter of transitivity need not have occupied him much beyond the observations noted in the first paragraph of the present section of this review. (7) is on a par, in logical status, with '$\sim fx$' or '$(x) \sim fx$' (as lately noted), and (3) is on a par with :

(8) $fx \therefore gx$.

They are on a par in the sense of being logical and non-valid and having some valid cases. Some cases of (3) are analytically valid, *e.g.* (4)-(6) and others not ; some cases of (8) are analytically valid, *e.g.* (2) and others not. For some choices of 'R', moreover, (3) is not analytically valid but still leads from true premisses always to true conclusions ; witness the Azores example. Correspondingly, of course, for (8).

7. *Further critical observations*

The direct value of the book is very considerable, and lies in the realm of logical analysis of ordinary language. The book also has additional value in an ironically negative way : the very misconceptions which I have been warring against in this review are philosophically significant enough so that it is important to have got them out into the open, particularly as they are probably not peculiar to Mr. Strawson. Finally it is scarcely to be denied that various proponents of modern logic have laboured from time to time under misconceptions of their own ; and some of those Mr. Strawson usefully sets right.

The value of the book in this last respect would have been enhanced if the author had made references to the literature. The anonymity of his "formal logicians say" engenders an air of *Strawson* v. *Strawman*. The discipline of documenting his adversary might also have operated now and again as a corrective, by leading him to wonder whether formal logicians do think quite the way he supposed they did, on certain points, after all. The almost total absence of citations has also other disadvantages, apart from the polemical point. Finding so much in the book that is familiar but unattributed, a less than omnivorous reader is in danger of supposing that the unfamiliar parts are old too, thus giving the author less than his due. Perhaps the ultimate in non-citation occurs on page 99, where it is said that Whitehead and Russell's fifth truth-functional axiom is superfluous; mere mention of Bernays would have enabled the curious reader to look up the proof, with help say of Church's *Bibliography*.

The remainder of this review will be given over to a series of miscellaneous points of criticism, each of which can be covered in briefer space than those belaboured in the foregoing sections.

There is a recurrent notion among philosophers that a predicate can be significantly denied only of things that are somehow homogeneous in point of category with the things to which the predicate applies; or that the complement of a class comprises just those things, other than members of the class, which are somehow of the same category as members of the class. This point of view turns up on pages 6, 112, and elsewhere. It is part and parcel of the doctrine that 'This stone is thinking about Vienna' (Carnap's example) is meaningless rather than false. This attitude is no doubt encouraged by Russell's theory of types, to which, by the way, Mr. Strawson seems to think modern logic is firmly committed (*cf.* p. 227). It is well, in opposition to this attitude, to note three points: the obscurity of the notion of category involved, the needlessness for formal logic of any such strictures on negation and complement, and the considerable theoretical simplifications that are gained by lifting such bans. This is not to deny the importance for linguistics of what the linguists call substitution classes, and at points Mr. Strawson has essentially that notion in mind (*cf.* p. 226); but the needs and purposes of linguistics are very different from those of formal logic.

On page 16, the author writes: "To say of two statements that they are contradictories is to say that they are inconsistent with each other and that no statement is inconsistent with both

of them". But this is unsatisfactory where S is by itself inconsistent, and hence inconsistent with every statement; for then every statement would qualify as a contradictory of S by the quoted definition, and thus contradictories of S would fail of mutual equivalence. The definition of "contraries", on the same page, is subject to a similar difficulty. My criticism depends indeed on assuming that a self-inconsistent S counts as a statement, but I think I am authorized in this by the foot of page 8.

A related difficulty occurs on page 87, where 'if' is being contrasted with '$\supset$': "As an example of a law which holds for 'if', but not for '$\supset$', we may give the analytic formula '$\sim$ [(if p, then q) . (if p, then not q)]'". But how does this supposedly analytic formula fare when 'p' is taken as '$q \,.\sim q$'? Presumably 'if $q \,.\sim q$ then q' holds, as a case of 'if $q \,.\, r$ then q'; and similarly for 'if $q \,.\sim q$ then $\sim q$'. Maybe the author's defence would be that my instance is one where, for ordinary 'if', the question of truth "does not arise" (*cf.* § 3 of this review); if so, then the passage needs expanding.

Whether or not the above two paragraphs bring out two genuine cases of failure to allow for an always-false component, at any rate just such an oversight does unequivocally occur on page 204. An assertion on that page hinges on incompatibility of:

$$(x)(y)(z)(fxy \,.\, fyz \,.\supset fxz), \qquad (x)(y)(z)(fxy \,.\, fyz \,.\supset \sim fxz),$$[1]

whereas actually both of these formulae come out true if $(z)(y) \sim fxy$.

On page 17, line 18, 'both' should be read 'each of' to avoid ambiguity.

In the italicized definition of 'truth-functional' on page 66, the words "*and only*" should be dropped. If they add anything, what they add is wrong; for we can often know the truth of a truth-functional compound without knowing the truth value of any component. A similar remark applies to "solely" in the middle of page 69.

On page 66, and again on page 216 (quoted in § 3 above), the idiom 'value of a variable' is used, contrary to custom, to refer to substitutable constant expressions rather than to the objects in the universe of discourse over which a quantification ranges. The latter, more orthodox usage occurs at the foot of page 112. On page 66 the "variables" are unquantifiable statement letters; so in this case it would be more natural not to think of

[1] Here and elsewhere, even in quotation, I depart slightly from the author's dot conventions.

them as taking " values " at all, but to speak of them as standing for (*i.e.* in place of) sentences.

Mr. Strawson is good on '$\supset$' and 'if-then'. He rightly observes the divergences between the two, and stresses that '$p \supset q$' is more accurately read as 'not (p and not q)' than 'if p then q'. He also shows awareness that such correspondence as '$\supset$' does bear to 'if-then' is better than its correspondence to 'implies'. But both ideas languish. Pages 218 ff. would seem less strange and more obvious if he would there revive the reading 'not (p and not q)'. Again the terminology 'material implication and equivalence', which he rightly deplores on page 94 but continues to use, could easily have been omitted from the book altogether in favour of the less objectionable terminology 'material conditional and biconditional', whose currency in the literature is encouragingly on the increase.

On page 106, " or doctors " should be changed twice to " and doctors ". The reason is that the logical sum of classes is represented rather by 'and' than 'or' in ordinary language, as the author has correctly noted on the preceding page.

In the small print of page 124 the author speculates on the possibility of a mechanical routine for testing validity of truth functions of formulae of Boolean class algebra, without remarking that the literature contains various.[1] Mostly these techniques, as published, are geared to the notation of monadic quantification theory, but they are easily adapted to the other notation. Actually the author is speculating on the possibility of a test of a somewhat special form; still the reader should be informed that tests are at hand.

On page 140, 'Nobody loves without somebody else suffering' is wrongly rendered '$(x)(\exists y)[\sim(x = y) . fx . \supset gy]$'. It should be '$(x)(\exists y)[fx \supset . \sim(x = y) . gy]$'. Mr. Strawson's formula is a logical truism, provable thus: $x = x$; therefore

$\sim(x = x) . fx . \supset gx$; therefore $(\exists y)[\sim(x = y) . fx . \supset gy]$.

On page 149, where the author explains Russell's theory of descriptions, the paraphrase which he gives of 'the King of England smiled' is redundant: 'x is King of England' can be deleted, for it follows from the ensuing quantification. Or, if he wants to keep the redundant clause for perspicuity, he might as well weaken '$\equiv$' to '$\supset$' in the ensuing quantification. The same criticism applies to page 185 and again to page 186.

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[1] For one and reference to others see *Methods of Logic*, p. 116 and preceding pages.

II.—CROCE AND HIS AESTHETIC

By E. F. Carritt

I suppose that among our contemporary philosophers none, with the possible exceptions of Bergson and Bertrand Russell, have had such a wide reputation or so wide an influence as Benedetto Croce. Neither he nor Bergson, whom he criticised with sympathy, toed the line of our fashionable trend in philosophy. They might have made fewer faults if they had, but they might not have served their generation so well. I think Croce is at present wearing better than Bergson, at least in the circle of the humanities. His essay on *The Historical Method and Karl Marx* is, as all 'listeners' know, still topical, and his criticisms of the Hegelian dialectic and of Fascist rule have scarcely ceased to be so. He was a Senator and, under the monarchy, Minister of Education. Under Fascism he published an outspoken essay on *Liberalism and Democracy*, which above any less famous signature must have incurred penalty. His old friend Gentile, who had become the composer of Mussolini's speeches, now attacked him with great acrimony in spite of disarmingly good-natured replies.

The reason for this popularity is clear. Croce was a real humanist, ready and sometimes over-ready to apply his philosophical analysis to religion, morality, political theory and, above all, art, as well as to logic. And there were special reasons why his work, particularly on aesthetics, was widely canvassed in this country. The *Aesthetic*, the first of his three volumes on the *Philosophy of the Spirit*, was published in 1901, the year of Andrew Bradley's inaugural lecture on *Poetry for Poetry's Sake* from the chair of poetry in this University. I believe I read the book and heard the lecture within a twelvemonth. Both greatly influenced the study of aesthetics here and had much that was common in outlook and insight, though with interesting differences. A few years later, the *Aesthetic* was the subject of Arthur Balfour's Romanes Lecture and in 1918 of a communication to the British Academy by Bosanquet, the populariser in this country of that Hegelian theory of the state which Croce had so effectively criticised. In the 1929 edition of the *Encyclopaedia Britannica* Croce contributed the famous article on Aesthetics which with his *Breviary of Aesthetic* he considered to contain his maturest views. In 1935 he was

elected a corresponding Fellow of the British Academy. In 1938 appeared Collingwood's *Principles of Art*, the best English book of its time on the subject and mainly an implementation of Croce, though from the point of view of an artist rather than a writer. Collingwood also translated his *Introduction to Philosophy* with a preface by Professor J. A. Smith.

Croce's most intimate connexion with this University, however, was when in 1933 he spent some time here as the guest of his friend J. A. Smith, when he captivated us all by his learning, originality and personal charm and by his Philip Maurice Deneke lecture on Shelley's *Defence of Poetry*, from which I must quote the last paragraph :

> It may be said in discouragement of vain hopes and over-confidence that the world is hard and heavy and needs more than individual good will and poetical fancies. But we know that this world, hard and heavy though it be, moves none the less, and indeed that it only exists in movement, and that it is moved by nothing but our united efforts ; and that each of us great or small or tiny as he may be, is, conjointly with all the others, answerable for the world. If we too, as lovers of poetry, exert what strength we have, we shall have done the duty of our station.

This stubborn individualism and modest optimism were the notes of Croce's message in years when these attitudes were harder to maintain.

I must now come to my main subject, the Aesthetic writings themselves, and I will begin with my agreements. I do not think I need dwell on his fundamental conception of aesthetic experience as what he calls an ' autonomous value ' or ' category of the spirit ', that is to say, an end and not a means, the handmaid neither of pleasure nor of philosophy as Hegel and others have thought, nor of religion, nor of politics as some think today, nor of science, nor of morals. All these are in a sense its subject matter, but art is for art's sake. We do recognise a genuinely common and distinct character in all the experiences we call aesthetic ; in ordinary language whenever we call anything beautiful, or artistically good. So much I hope is common ground. Certainly it is not new.

What struck and attracted me in the *Aesthetic* when I first read it was that it seemed the explicit and reasoned statement of what nearly all philosophers and critics had from the beginning fumbled after and what the iconoclastic devotees of abstract art still see darkly in a glass. I say from the beginning because Plato and Aristotle confessed that music, with the other unrepresentative arts, and natural beauty were expressions, or, as they loosely said, likenesses or imitations of moods and characters

and movements in the soul. I shall not offer a history of aesthetic, but we find the same tendency working to the surface through all the theories of imitation, pleasure, utility, morality, instruction and mystic revelation. I could easily mention thirty eminent names among forerunners [Longinus, Plotinus, Bacon, Dennis, Shaftesbury, Addison, Kames, Reynolds, Reid, Alison, Kant, Wordsworth (emotion recollected in tranquillity), Coleridge, Schelling, Shelley, J. S. Mill, Hegel, Ruskin, Pater, Nettleship, Bosanquet, Tolstoy, Santayana]. But I will mention two later writers because I do not think they were influenced by Croce. Clive Bell and Roger Fry both speak of 'significant form', which must be significant of something. An eloquent contemporary eulogist of abstract sculpture says that it expresses the rhythm of the universe; but as nobody can know the rhythm of the universe, any more than the music of the spheres, this can only mean the expression of the sculptor's emotions at what Wordsworth more modestly called its 'goings on'.

Croce's theory of beauty is commonly summarised in three words: "expression of emotion". In more words it is a language which portrays an emotion in a sensuous form. But this has to be made more precise. He distinguishes expression from mere symptoms like blood pressure, from emotive infection like rhetoric, and from conventional symbols like the swastika or the sickle. He also distinguishes emotion from mere sensation or, as Collingwood, I think, called it, 'commotion'. Examples of emotion would be love, hate, grief, hope, gaiety, devotion; of commotion, brute desire, shock, blood-lust, panic fear and agony. All these we may call sub-human states. Croce is misleading or confused at this point because for the first or subconscious stage in the process of presentation he, like Hume, uses the word 'impression' though, as Hume more explicitly says, with no implication of any external cause. By the time this has become an 'emotion' it would seem already on the way to 'expression'. He is indicating the difference which Gray described in writing to West when he speaks of the black melancholy in which he could create nothing, whereas the 'leucocholy', or white melancholy which had always 'marked him for its own' issued in the Elegy. There were times when Cowper could write nothing, not even—

> No voice divine the storm delayed,
> No light propitious shone,
> When snatched from all effectual aid
> We perished each alone:
> But I beneath a rougher sea
> And whelmed in deeper gulfs than he.

With this summary phrase 'expression of emotion' I quickly found myself in agreement. And Croce soon converted me to a second point necessarily following from it, though at first it seemed paradoxical because less anticipated by his predecessors. If beauty is a meaning or expression it cannot be a quality intrinsic to the object called beautiful but must be a relation of it to some person or persons as is utility or belovedness or intelligibility or strangeness. A word or sign has no expressiveness in itself but only for those familiar with the language, the code or the grimace. And then its precise meaning for them will depend on their nature, education and general history and indeed to some degree on their present mood and the exact context of the situation. This corollary of expressionism, I say, had not been always recognised, though it was by Hume, Richard Price, Kant and later by Rashdall who perhaps derived it from the last. But if this subjective theory had not a very long pedigree it has in recent times found an unexpected bedfellow. One article of logical positivism is that aesthetic judgments, so-called, mean or assert nothing but are merely emotive or expressive of feeling. I should myself go further and hold that *no* collection of sounds or ink-marks in themselves mean anything but only that we who use them often do and are often so understood, and sometimes we find a meaning where none was meant. And I am sure that in my unregenerate days when I called a flower or a picture beautiful, I meant that it had that quality as truly as it had its shape. But I have come to think, with Croce's help, that I was wrong, and that all I am entitled to say is that it stimulates in me and perhaps others the kind of experience I call aesthetic: to call a thing beautiful is for me a statement about my experience; to make or imagine a beautiful thing, or to appreciate one, is to express my experience. Logical positivists started from an alleged law that no sentence can have meaning unless it is either tautological (as they thought all self-evident truths are) or probabilifiable by sense-perception. Clearly the sentence 'X is beautiful' satisfies neither of these tests. Moreover it is not self-evident as I myself think some sentences about obligation are, though they are not tautological. So Hume, as often, was right about aesthetic judgments but, as sometimes, wrong about moral ones, at least if I am right. This I take to be a fundamental difference between morals and aesthetics, the neglect of which has injured both.

A third point in Croce which I value is his insistence on the tradition, going back at least to Longinus and Plotinus, that

aesthetic theory must take account of nature as well as art, since our experience in face of both is of the same kind and may be indistinguishable. Many theories have shipwrecked by failing on this point and Croce's candid recognition of a truth so testing for his own view was courageous.

So much for my agreements. There are, however, two doctrines strongly emphasised by Croce, but, as I think, quite extraneous to his main thesis, and which I cannot accept. He regards them as interdependent; I find them incompatible and both false. The first is the attempted integration of the aesthetic to a subjective idealist metaphysic. The other is the identification of expression with 'intuition', by which word he means any internal or external presentation. Minor and more questionable points are his rejection of the so-called Kinds of beauty, his assertion of the indifference of the Subject, and his denial of degrees of beauty.

I will begin with my two main points of difference, which are so closely connected that they must be treated together. As to the metaphysic, Croce says 'It is only on a dualistic hypothesis that the so-called physical object can exist except in an individual mind.' And he applies this dogma to what are generally called works of art as follows: 'Those who suppose two forms of intuition (or presentation) one objective or physical the other internal or aesthetic, the one impressed from without the other expressive of the depths of the soul, offer but a vulgar aesthetic . . . which is the suicide of philosophy.' Any sensum or image present to the mind, whether we refer it to a physical object or not, must then be purely self-created. It is only on that condition that it is beautiful, and every sense-presentation is beautiful. How on this theory can Croce account for the communication of aesthetic experiences from an artist to his public? I must ask for patience if I give his answer (*Estetica* XVI) in some detail. He represents the parties to the transaction and their several activities by a number of alphabetical symbols (Aα, Bβ and C) which I find very confusing. I shall therefore replace them by proper names; and since not even Philalethes and Alciphron are always easy to distinguish, nor even Glaucon and Adeimantus, I shall use the real names of the parties to a historical transaction no doubt familiar to many of my readers. With such substitution this is Croce's account of a communication:—Gerard Manley Hopkins once had an emotion which he expressed to himself internally by a poem, and this was the complete work of art. He then externalised or communicated it to Robert Bridges who subsequently had

an emotion which he expressed internally in an aesthetic experience of his own, and Croce argues or rather assumes that the experiences of the two men were, or might have been, closely similar. But it is hard to see why. For on Croce's view nothing physical such as inked paper, coloured canvas or air-vibration intervenes. Apart from thought-reading there is no reason to suppose that Bridges's experience was in any way connected with that of Hopkins. Between the bounds of different minds there is indeed, on this view, 'the salt, unplumbed, estranging sea'. Croce's only escape from this blind alley (short of adopting the 'dualistic hypothesis') would have been to embrace an absolute idealism, either one like Hegel's in which the universal Man is more real than the individual men, so that there is no trouble about thought-reading, or else one still more extreme like Gentile's, where other minds and the past and future are alike relegated, along with matter, to the limbo or inane of dead thought, *pensiero pensato* or illusion, and the only reality is the monochronous act of *pensiero pensante*, so that neither history nor second-sight come in question. But neither of these escapes was possible for Croce, the apostle of individual personality, alike in history, philosophy, politics and art.

The truth concealed by these errors seems to be that the aesthetic experience arises and can be completed in an individual mind, though it is often stimulated by an artefact or by nature, and is usually aided by the artist's own scribbling or strumming. When it is completed it is what Wordsworth called 'the inward eye which is the bliss of solitude'. Croce calls it the work of art. I prefer to retain the term 'work of art' for the physical artefact by which the artist hopes to communicate something like his aesthetic experience. In this he may succeed with a public something like himself, and roughly in proportion to their imaginative capacities, though if these are better than his own so may be their aesthetic experiences. As John Stuart Mill said: 'Rhetoric is heard (and meant to be heard), poetry is overheard.' As Coleridge said: 'We receive but what we give and in ourselves alone does nature live.' That is equally true of an artefact, which is a dead thing though it once had aesthetic life in the artist's mind, a life probably more, but possibly less, vigorous than it now stimulates in our own.

What I have here called the underlying truth is what Croce calls 'a vulgar aesthetic' and 'a backsliding into the empiricism of the *Einfühlung* or Empathy school'. It does seem,

however, to give an intelligible account of the vulgar fact that approximate communication of an aesthetic experience can take place by means of a physical artefact. And I am on good enough terms with empiricism to employ it if it can do that. And I do not intend suicide.

It may be objected to such a view that if the artefact of a genius stimulates experiences similar to his in some who are similarly endowed and trained, it can be said to have an intrinsic or objective beauty, and that those who fail to appreciate it as he did are just wrong. But this is to beg the question. It is like Aristotle's appeal to an academic *élite* of *πεπαιδευμένοι* whose conditioned imagination is to be the test of truth. The empathy school has faults of its own, but I do not feel that Croce is on very firm ground in condemning it. My own suggestion is that approximate communication of aesthetic experience is possible in favourable cases—say within the same nation or culture and the like—but that how far the activity stimulated resembles that of the artist depends entirely on the recipient. This was always suggested by the differences of taste in apparently well-qualified judges, and has received confirmation from recent psychological experiments which seem to show that when the same coloured pattern is presented to different people they may 'see', as they say, very different things. After all, the question what an artist meant is not an aesthetic but a historical one. It would be hard if we might not admire ruined buildings, or the 'stretched metre of an antique song'. Certainly wild nature was aesthetically a very different thing to the eighteenth century and to ourselves. A further point against Croce here is that if we ever do become aware of anything sensuous which we think ugly or indifferent, it is unexpressive of our emotions, so that after all expression and intuition are not identical. Consider the case of being interrupted during reading by a jet-bomber. That intuition might cause us commotions but would it be expressive of anything already present in ourselves?

The usual objection to the subjective view, as I may shortly call it, a view which I share with Croce, is that it precludes any distinction between good and bad taste. To this I think he has a satisfactory reply. The ideally worst taste is to have no experiences of beauty, and the best is to have the most that are vivid and pure. And pure means unadulterated by non-aesthetic elements. Many things are called beautiful because they flatter our passions or our vanity or our curiosity. By the proviso of 'vivid' Croce would

exclude what Collingwood, paraphrasing him, called 'entertainment', covering a good deal of light opera, occasional verse, illustration, problem-plots, and of course many reviews, pantomimes, novels, plays, and films. In such there is no underlying passion; but expression is precisely what 'liberates' us from the tyranny of a passion, at least for the moment.

I now turn to some minor but perhaps more interesting points in Croce's *Aesthetic*. On the first I think he lays undue stress though it occasions some brilliant remarks. It is his rejection of the traditional artistic Kinds or labels within the aesthetic sphere, with their derivative Rules. He has chiefly in mind such distinctions as Sublime and Beautiful, Classical and Romantic, Tragedy and Melodrama, Heroic and Epic Poetry, Eclogues, Sonnets, Sonatas, Pindarics, the Pastoral and the Picturesque, all once supposed to have laws, such as the famous Unities, unfairly ascribed to Aristotle, which it was fatal to break. He contends truly that every work of art is an unique individual creation escaping the meshes of any such nets. These 'Kinds' he calls Pseudo-concepts, contrasting them, perhaps on a Kantian analogy, as empirical universals, with true 'categories', of which he allowed seven: Spirit, its two activities the theoretical and the practical, each subdivided into two forms or grades, the theoretic into aesthetic and logical, the practical into economic or utilitarian, and moral. He admits of course that the pseudo-concepts are handy practical devices like arranging your books by size; he only holds that they are aesthetically irrelevant, and not to be set in a hierarchy of precedence or with prohibited degrees of intermarriage. "All the kinds are good except the dull kinds", and the sublime may go hand-in-glove with the ridiculous. I think Croce was less concerned to deny the existence of aesthetic species than to allow, like Darwin, the constant and unpredictable origin of new ones. He rationalised the Classical-Romantic antithesis as referring not to two kinds of beauty but to two elements in every aesthetic experience, the emotion or matter and the expression which gives it form. If the emotion is too crude to be quite mastered in tranquillity we get romantical, or, as Santayana and some eighteenth-century critics of Shakespeare called it, barbaric art; if it is too faint for the elaboration lavished on it we get classicist or polite or academic art. Instead of the aesthetic experience, at the one extreme—the barbaric—our flesh is made to creep, at the other extreme—the classicist—it is the 'slow lines that creep' and 'we cannot blame indeed

but we may sleep '. I must quote the passage from the *Breviary of Aesthetics*:

> What, ask the Romantics, is art worth, for all its polished imagery, if it does not speak to our hearts? And if it speak to our hearts, what matter if its imagery lacks polish? And their enemies will answer them: What is all this emotional excitement worth when the mind can find no lovely image to rest on? And if the imagery be lovely, what matters the lack of a passion attainable without the help of art, often showered upon us by life more freely than we could wish? . . . Great works of art are both classical and romantic, emotional and life-like, they are sheer emotion absolutely identified with the most lucid imagery. . . . Epic and Lyric and Drama are scholastic distinctions of what cannot be separated. Art is always lyrical just because it is the epic and drama of the feelings.

Was he thinking of 'Lyrical Ballads'? And he quotes with approval Pater's 'All art constantly aspires towards the condition of music' and Amiel's 'Every landscape is a mood' or, as Constable said, a poem. And on the other hand there is word-painting. And there is architectural rhythm. For even distinctions by medium, like that of Lessing between poetry and sculpture as dynamic and static arts, are pseudo-concepts. Shiva is all motion in bronze. On the sublime Croce says, 'Everything is sublime which anybody has ever called so'. I think he should have said, 'The term is appropriately used when hearers understand it to refer to experiences similar to those intended by the user'. Addison had said 'Those who deny that *Paradise Lost* is a Heroic Poem only mean that Adam is not Aeneas'. The contemporary French novelist Duhamel in his *Chronique des Pasquier* writes, 'At the same musical performance one hearer feels his joy suddenly accomplished, another's sorrow is enriched and purified, becomes at once intelligible and beautiful'. In fact the only relevant way of classifying aesthetic experiences is by their own quality. I wish Croce had rested his argument here on the critical inadequacy of the Kinds rather than on dubious logic. I also wish he had given more attention to the difficult question of the ludicrous: is it a pseudo-species of the aesthetic or a cognate species under some unnamed genus?

Croce points out that our contemporary version of the Kinds is the distinction by styles. It was foreshadowed by the sublimity of Longinus, by Schiller's *Naive and Sentimental Styles* and by the *Classical-Romantic* antithesis; but it became the mode with Nietzsche's Apollonian and Dionysiac styles, which were adapted by Hulme in his *Speculations* and also by Wöllflin, as the vital and geometric styles. And from these we have

passed through Impressionism, Imagism, Symbolism, Verism, Existentialism and so on, all of which, in theory at least, aimed at expressing not mental states but rather fragments of these as analysed by psychology, and in practice were often less expressive than affective or symbolic. Of such slightly comic psychologisms Croce quotes an instance from Proust, that of the remembered biscuit once given him when a child for tea.

On another considerable though minor topic in the *Aesthetic* I again agree in principle, but would express myself rather differently. Croce holds that the subject of a work of art is aesthetically quite irrelevant. This is what he says: 'One artist is in raptures before a smiling landscape, another before a rag-and-bone shop; one before the face of a pretty girl, and another before the squalid features of some old ruffian.' Here he differs from Hegel, who held that in a great work of art three elements, each of them aesthetic, must be so fused as only to be distinguished by analysis. They are, first, the subject-matter (*Inhalt, Sache*); second, the idea or presentation of this (*Vorstellung*), and third the technical rendering or style. Andrew Bradley criticised this by distinguishing what he called the subject from the substance. By subject he seems to mean sometimes the title, for instance *The Excursion*, and sometimes the stimulus, for instance the French Revolution or Annette Vallon. This he says is quite outside the work of art. The substance on the other hand is something inside the work of art, which has some aesthetic value and which we may be able to remember when we cannot recall a single verse or colour or note, felicities that may contribute more or less to the whole effect than the substance does. I think this so-called substance may be a faint prevision or recollection of what the total experience will be or was. If we remember something of the effect of Macbeth upon us, though none of the words, what we do remember would have been different if the words had been different. But I am still puzzled whether some subjects or stimuli may not be apter than others to evoke in an artistic mind emotions likely to issue in beautiful expressions. No doubt very bad poetry has been written about God or love or heroism or death but it may be possible to write greater poetry about such subjects than about 'The Rape of a Lock' or a game of ombre or a Sofa or a tea-biscuit. Catullus certainly wrote a very good poem which professed to be about the death of a sparrow—priced at half a farthing—but, perhaps because I know English much better than Latin, I think I know a better poem on the death of a child. Is Croce here consistent with his contention

that beauty must express emotion? He could consistently, and I think truly, maintain that any subject is a good one if, but only if, the artist and his public can feel it deeply. What led him to his exaggeration was the next and last of his paradoxes which I now propose to treat.

This is the doctrine that there are no degrees of beauty. If that is true we could never say that Catullus's poem is more or less beautiful than Bridges's or the Medici tombs than Della Robbia's *bambini*. I can sympathise with his motive here—his distaste for snobbish eulogies of important works of art, the grand style, heroic poetry, historical painting, monumental sculpture, philosophical and theological poetry, all the solemnity which can so often lead to dullness. He explains what is called an imperfect work of art as one with perfectly beautiful parts imperfectly fused into an attempted whole. He saw that a song in an ugly play might be beautiful, but would not allow that the one might be more beautiful than the other. His error seems clear if we take a different kind of instance. May not a ruin or a torso have more or less expressiveness than the original work? Or again, we may have thought a passage in Wordsworth's published *Prelude* beautiful before we knew his earlier version of it. When we read and prefer this, are we bound to say that our old favourite really expressed nothing to us at all, and that therefore, on Croce's theory, we did not really intuit it? Perhaps Croce would say that each was just a perfect expression of a different emotion. But if Wordsworth had thought so he would have published both. Croce himself has an interesting passage where he says that an artist, seeking the expression for his subconscious emotion, tries one phrase or pattern after another, now approaching, now receding from his goal till the creation is achieved. Is not then the nearer attempt more beautiful? Or is it only less ugly? It would seem strange if there were degrees of ugliness but only perfect beauty —if earth had not anything to show either more or less fair. But I must repeat that I do not see how on Croce's theory we ever intuit anything ugly. Yet to deny that we do is wishful thinking.

I will now try to summarise shortly my agreements and differences with Croce's *Aesthetic*. And first what I take to be its fundamental truths. All rational beings have some aesthetic experiences, and on reflection we recognise these as distinguishable from others and we call what excites them beautiful. Beauty is the projection of them into sensa when we contemplate these imaginatively, that is to say without reference to their

reality and without practical, scientific or historical interest. All beauty, like that attributed by lovers, lives in seeming. Aesthetic experience is not practical, either moral or hedonistic, but contemplative or theoretic. Yet not all theoretical activity is aesthetic, only the imaginative or intuitive. This is what Schiller rather ineptly called 'play' as distinct from moral, prudential or controversial activity. Kant called it the 'free harmony of the imagination and the understanding', or 'perception without a concept' or 'purposiveness without purpose'. Why then do we value this butterfly expression? What does it express? What is its significance? Croce answers, rightly I think, that it is expressive of our hidden hearts, or rather of human hearts more like our own than perhaps we had been ready to admit. And what could be more interesting? This was Shelley's defence of poetry. It is psychology by acquaintance not by description. It is what so many previous theorists had tried to say about the relation between knowledge and feeling in the aesthetic experience.

So far I agree; but I think the idealist metaphysic and the equation of intuition with expression are mutually inconsistent and both false, as well as irrelevant to the main thesis. I think the attack on the Kinds was valuable to criticism but not happily stated, and I should say the same about the denial of importance to the subject. I cannot accept the denial of degrees of beauty though I sympathise with its motive.

But one may owe something to a teacher even when one disagrees with him, and when one has learnt something one owes everything. My debt to Croce would not allow me to end on the note of cool criticism, ready as he always was to welcome it. My personal tribute to him may be thought ambiguous as applied to a philosopher but I believe he would have appreciated it. He is one of the very few who, merely by their philosophical writings, give me a feeling of something like affection so that, as Socrates felt for Homer, 'hereafter, in a better world than this, I should desire more knowledge of him'. His essay entitled *My Philosophy*, published in 1945, after extenuating the possessive pronoun, ends with these words:

> As to the future of this so-called philosophy of 'mine', like all others it will be superseded (as I have often superseded it and shall so long as I live and think); it will be superseded by the *unda quæ supervenit undam*, by the growth and widening of the human spirit. And yet the truths which I have been able to discover and to establish remain and will remain. Great men or little men, we can neither hope nor look for more than that.

And I feel I must add, as a tribute to his work in other fields than aesthetic, the conclusion to his *Soliloquy of an Old Philosopher* published in 1942:

> A man whose mind is thus religiously disposed gladly leaves the care of political history to the politicians and soldiers and economists. He fixes his thoughts on moral history, where is unrolled the drama that also goes on in himself, and where, throughout the centuries, he meets his fathers and brothers, who loved liberty as he does, and, like him knew how to work and to suffer for her.

His best funeral eulogy is that he in fact had two. One from the Catholic Church and one from the Communist Party; both of course guarded but both warm. If we consider the close friends he had in both camps and his sympathetic criticism of their several doctrines we can say of him as a man what Antigone said of herself:

οὔτοι συνέχθειν ἀλλὰ συμφιλεῖν ἔφυ.

To quote a characteristic saying of his own: 'The man has a real life to live, the life of classes is a fiction.' That was his attitude in history, in political theory and in art.

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III.—ON THE DIRECT PROBABILITY OF INDUCTIONS

By Donald C. Williams

Perhaps the oldest, and anyhow the only *logical*, proposal to 'justify' inductive inference has been to show that when we infer from a premise which statistically describes a sample to a conclusion which similarly describes the wider population from which the sample was drawn, the conclusion has a high 'probability' of the sort defined by the most familiar and least objectionable form of the classic theory of probability: in brief, the 'chances' are overwhelmingly in its favour, as they are for the drawing of a white ball from a bagful which are nearly all white.

From Bayes to Carnap, this has been accounted a venture in 'inverse' probability. When our evidence includes the composition of the population in a certain respect (that half the cards in the pack are red, let us say) we can easily figure a 'direct' probability for the conclusion that a hand of eight cards, say, will be half red, or approximately half red, as we prefer. In the inductive situation, however, we seem to be starting at the other end; our evidence includes the composition of the sample or hand (half red, or all red, or whatever), and we want the probability that the population or pack has a similar composition (is half red, or all red, or whatever). In the traditional logic of chances, the rule of Bayes has promised to 'invert' the probabilities provided for inferences from population to sample, to yield a probability for the inference from sample to population. Specifically, the probability, given a sample with composition p_1', that the population composition has any value p_1 (in particular, that this is the same as p_1'), is equal to

$$a_1 \times \mathrm{P}_1 / \Sigma(a \times \mathrm{P}),$$

where a_1 is the antecedent or '*a priori*' probability that the population would have the stipulated composition, P_1 is the direct probability, given a population with composition p_1, that the sample would have composition p_1', and $\Sigma(a \times \mathrm{P})$ is the sum of such products for all the possible values of p. The antecedent probabilities a can plausibly be said to be all equal —the population is antecedently as likely to have any one composition as any other. Then they cancel out, and our net inverse

probability is $P_1/\Sigma(P)$, which is calculable by the arithmetic of Bernoulli. If the sample is large, the result is a high probability, not that the population has exactly the observed composition, but that it has approximately the same.

The philosophers who have echoed Hume on the irrationality of induction have all too seldom examined this principle or even known of its existence. Laplace, Jevons, and now Professor Carnap—no mean company—have accepted the inversion as competent to the inductive job. But while no knowledgeable person disputes the validity of the formula *per se*, its usefulness for induction has been frequently denied. Aside from those who will have nothing of the logical concept of probability at all, Lord Keynes, Lord Russell, Professor Jeffreys, and Professor Broad have used the principle very warily, eked out with material assumptions, and Mr. William Kneale, in his excellent *Probability and Induction*, flatly disowns it. The sticking points, aside from certain paradoxical consequents which doubtless can be allayed, are the inscrutable idea of the *a priori* probabilities *a* and the arbitrary assumption that these are all equal.

The conviction, however, that if there *were* a logical probability for the inference from the sample to the population, it would have to be the inverse of, and presumably different from, that for the inference from the population to the sample, remains generally unimpaired. Keynes, Russell, Jeffreys, and Broad have despaired of a probability for induction just in the proportions in which they were persuaded that the inverse-probability formula is ineffective, while Mr. Kneale, who is wholly persuaded, wholly despairs. Statisticians, partly ignorant of and partly disillusioned with the inverse rule, have been avoiding the concept of an inductive probability, preferring an *ad hoc* notion of 'reliability', or Professor Fisher's 'fiducial' criteria, or the cognate theory of 'confidence' of Professors Neyman and Pearson. Yet the logical good sense which, four times out of five, is a better guide than our spasmodic computations, is revolted that the very high direct probabilities with which the rule of Bernoulli hallows any inference from a population to a (large) sample should be without significance for the inference from the sample to the population. Statisticians in fact went right on using those probabilities, by one device or another, as measures of the propriety of their inferences. One device was simply to rechristen the direct Bernoullian probability "the reliability" of the opposite inference. The methods of Fisher and Neyman, triumphant in Britain and America now, while

renouncing any logical or philosophical rationale, calmly accept or reject statistical hypotheses concerning a population in accordance with what Fisher calls the 'likelihoods', that is, the relations of direct probability which run from the respective hypotheses to the sample observation; and Mr. Kneale has lately appended these floating rules of thumb to his own prescriptions of inductive 'policy'.[1] It seems queer that such operations should be so attractive in principle and so successful in practice, yet be altogether unsanctioned by the calculus of probability. In particular, it is bizarre that there should be no affirmative implications for a logic of induction in the fact that we not only are able, given the population composition, to calculate the probability of a sample's having the same composition, but are able to assert with complete generality that if the sample is fairly large, then for *every* composition p which the population might have, there is a demonstrably high probability that the sample composition p' will be approximately the same.

I have argued elsewhere that these logical circumstances are sufficient and conclusive: that they establish for the inductive inference a *direct* probability of ample if somewhat indefinite degree. Thus the vagaries of probability inversions are avoided, while to reduce to the utmost any appearance of legerdemain, I simplified the issue by dealing solely with ratios of numbers of actual objects in the one real world, so that instead of talking at large about the probability that a sample set taken from a population will match the population, I dealt with the actual proportion of matching sets among all the sample sets included in any given population. The justification of induction which I proposed, to be taken either as a logical ground for crediting inductive conclusions or as an analytic or *a priori* explanation why inductions generally succeed, can be summed up as follows: "Since we know *a priori* that the great majority of sets [or possible samples] which are choosable from a population are statistically similar to that population, it is highly probable that the group which we actually draw as a sample is statistically similar to it, *and hence that the population is statistically similar to the sample* . . . Every sample, in other words, is like a chip, marked with a fractional number, drawn from a bucket of chips among which we know most of the chips are marked with the true composition of the population in which we are interested."

Mr. Kneale quoted this passage in a review, with a sombre

[1] W. C. Kneale, *Probability and Induction*, Oxford, 1949, Part IV; J. Neyman, *First Course in Probability and Statistics*, New York, 1950.

motive.[1] He gave the emphasis to the underlined clause because he believed that right there I committed a fallacy which would, indeed, have stopped so still the heart of my philosophy of induction that I wondered at the kindness with which he treated the corpse. The fallacy was that, in diametrical contrast to my wearisomely advertised intentions, I 'covertly assumed an oversimplified principle for the inversion of probability statements'. This diagnosis I believe was mistaken. As it turned out, no other *logical* fault has been charged against me, but this one has been charged independently by several accomplished persons, in print and in correspondence. Other reviewers and commentators, to be sure, were not won round by my argument, but their objections were on other than logical grounds. They complained, for example, that the problem of justifying induction is a pseudo-problem, to be obviated either by rational insight into real causal connexions, or by compilation of ordinary language, or by convention or animal impetus; or that my solution couldn't be correct because its results were too good to be true, or because it transgressed the dicta of David Hume, or because a 'logic of induction' is a contradiction in terms, or, much the most frequently, that a predominance of chances such as is provided by the classical theory and the bagful of balls, is not really a rational incentive to believe not ever, or anyhow not when part of the bagful is in the future. Mr. Kneale, however, does accept as fundamentally valid, in the finite world, the classical probability yielded by the balls. Ostensibly, then, the only thing which prevented him from accepting my doctrine of induction for the finite world (in distinction from his 'open classes' which I believe are fictions) was the objection he raised in his review. The lapse of many months and the appearance of several important books with different ideas on the logic of induction have perhaps increased the intrinsic interest of the topic, and I should like to revive it by examining the supposed logical difficulty and to defend the thesis of the direct probability of inductions by showing the reproach to be undeserved. Mr. Kneale's stricture can be our explicit focus, because it is concise and accessible, but the discussion will ramify a little to touch on some variations.

[1] *Philosophy*, Vol. XXIV, 1949, pp. 86-88. The quotation is from Donald Williams, *The Ground of Induction*, Cambridge (Mass.), 1947, pp. 98-99. Incidentally Mr. Kneale disputed my notion of the dire cultural consequents of inductive scepticism. An interesting later documentation of this notion is by J. W. H. Rosteutscher, *Die Wiederkunft des Dionysos, der naturmystische Irrationalismus in Deutschland*, Bern, 1949, whose first paragraph is about David Hume.

Mr. Kneale uses "probability" for the relative frequency of a property in a class and not for the corresponding relation of propositions, as I should, and he writes "$P(\alpha, \beta)$" for the 'probability' that a member of the class α is a member of the class β, that is (if α is finite), for the relative frequency of βs among the αs. He introduced special symbols for our topic with the following legends:

σ: sample from a population of M things;
δ_p: drawn from a population which contains a proportion p of P things;
$\epsilon_{p'}$: exhibiting a proportion *approximately* p of P things.

He then compared the formula (1) "$P(\sigma\delta_p, \epsilon_{p'})$ is high" with its 'inverse' (2) "$P(\sigma\epsilon_p, \delta_{p'})$ is high". Sheer substitution of the defined equivalents for the symbols is awkward, but we can translate and expand as follows:

(1*a*) Among the sample sets included in a population which consists of things of the sort M, and in which there is a proportion p of things of the sort P, the relative frequency of sets containing things of the sort P in approximately the proportion p is near to 1.

(2*a*) Among the sample sets which may be drawn from a population consisting of things of the sort M and which exhibit the proportion p of things of the sort P, the relative frequency of sets from populations which contain things of the sort P in approximately the proportion p is near to 1.

Now, said Mr. Kneale, the first of these expresses the gist of "The great majority of sets . . . choosable from a population are statistically similar to that population" and is unexceptionable; but the second gives the gist of, or anyhow is required to vindicate, the fatal clause which he italicized. By the quick exchange, 'it is made to appear that the second assertion is merely a rewording of the first. Since it is highly probable that a sample will be "statistically similar" to the population from which it is drawn, therefore, . . . it is highly probable that a population will be "statistically similar" to a sample drawn from it'. The procedure, in other words, 'equated the proportion of possible samples from a population of given composition which are "statistically similar" to that population with the proportion of possible samples of given composition which originate from "statistically similar" populations'.

Other good critics too have scented a trick here, and no matter what the outcome of the present discussion, the very indictment convicts me of a deplorable failure of exposition.

For this point was where my attention was concentrated, and the spell of the description of induction to which Mr. Kneale resorted is what I thought had long baffled us all and what I tried diligently to dissolve. Perhaps I can do somewhat better now, however, with the articles of the accusation before us.

Our webs of ratios, our overlappings and inclusions of classes, and our classes of classes, with their various dimensions of generality or scopes of quantification, are a little intricate, and neither Mr. Kneale's formulas nor my paraphrases are unequivocal. Let me put the first principle in a form to which I am willing to commit my inductive philosophy, making it at the same time a bit more specific in its statistical arithmetic and using "p" and "p'" in a way slightly different from Mr. Kneale's—all this for convenience of some later applications.

> (1*b*) For every class M and for every class P, no matter what the proportion p of P among the Ms, at least 95 per cent. of the sample sets consisting of 10,000 Ms apiece must contain P in a proportion p' which is equal to $p \pm 0.01$.

Notice now that the last pair of Mr. Kneale's colloquial redactions are inaccurate in that he suggests that to know the predominance of the sample sets which match M, and hence the stated probability that any one such sample set will match it, we must have among our premises a statement of p, the actual composition of M with respect to P (or, in the inverse case, p', the actual composition of the sample with respect to P). My correction of (1*a*) makes plain that even for the admittedly direct use we can assert *a priori* the slightly indeterminate predominance and probability, "at least 95 per cent.", altogether independently of the value of p, so that p does not enter into the probability statement at all: it can be left a variable, unspecified, until the question of probability is settled.

This is cardinal to the whole argument. There is, to be sure, a sort of direct inference whose premise does state the composition of the population, while its conclusion states the probable composition of (say) a myriadfold sample from it. If the population is very large (as we are assuming it to be, for the sake of simplicity, not of probability), one form of the rule for it is that in 95 per cent. of the sample sets, $p' = p \pm (2\sqrt{p \times 1 - p}/\sqrt{n})$, where n is the size of the sample (and so on for other specific percentages for other degrees of approximation). Thus if we have in evidence that the proportion of American voters who are for Polk is 0.43, we can say right off that there is a probability of 0.95 that a sample of 10,000 will contain Polkians in a

proportion $0.43 \pm \sqrt{0.43 \times 0.57/50}$, that is, between 0.4201 and 0.4399. My rule or procedure, however, is not this one but a slightly indeterminate corollary of it which takes account of n but ignores and is independent of p by grace of the fact that the poorest possible approximation compatible with the general formula is given when the quantity under the long radical sign is equal to 0.25, whose root is 0.5, whose quotient with 50 is our 0.01. A person might make a 'direct' use of this corollary even though he knew the composition p, simply to save time, saying, "I won't bother to multiply or to extract roots: there is anyhow a probability of at least 0.95 that the selection will evince a proportion 0.43 ± 0.01". But then why may he not make the same 'direct' use not only without using p but without knowing it? Imagine us hurriedly dividing up a pile of apples, for example, before the frost gets them. Some are wormy and some are not—we don't know which or how many. But each of us is rationally content to take his 10,000 at random because he knows that there is a high probability, at least 95 per cent., that the worminess in his batch won't deviate by more than a paltry 100 apples from his proper share or quota as we could have determined it by counting over all the apples. But if we can say this, surely, we can reckon backward, some long winter evening, and establish how many wormy apples there were in the pile. Counting over my own 10,000 and finding, say, that 3500 are wormy, I can chalk up that, with a probability of at least 95 per cent., the proportion of worminess in the orchard's whole product was between 0.34 and 0.36. Just thus, I think, do we progress from an indisputable 'direct' probability to a plainly inductive one, and I challenge the critic to say where an 'inversion' occurs, illicit or otherwise.

First, now, let us go back and ask what is the inverse principle which Mr. Kneale thought I covertly used, and secondly, let us examine my attempted formulation of the direct probability of inductions, in slow motion, so to speak, to discern whether it involves any such inversion.

It proves oddly difficult to state unequivocally any inverse principle, true or false, which can plausibly be read into Mr. Kneale's formula and supposed to justify inductions. Traditionally, simple inversion has consisted in equating the probability of a sample composition $p \pm x$ in relation to a known population composition p, with the probability of a population composition $p' \pm x$ in relation to a known sample composition p'. But Mr. Kneale and I agree on asking a strict statistical basis for our probabilities (so far as concerns the finite world) in

the overlapping of actual classes, and what statistical rule is he proposing that I require? We have confronted a prodigious tangle of statistical relations among which many might be regarded as the 'inverse' of the one on which we have agreed. The subject or 'reference class' of the inverse principle which Mr. Kneale proposes, however, is rather plainly to consist of sample sets exemplifying the single composition, namely, p', exemplified by the sample drawn. But by what other criteria is it determined? The sets are said to be from the population M, but also to 'originate from . . . populations', as though they embraced sample sets of M which are representative of other populations too, or even sets representative of other populations but not of M.

Let us first try considering no population but M. Using "myriadic" to mean ten-thousand-membered, we have:

> (2*b*) At least 95 per cent. of the myriadic sample sets included in M and evincing P in the proportion p' come from a population, namely, M, in which the proportion of $P = p' \pm 0.01$.

In one way of understanding the quantifications here, this might mean that the proportion of P in any population M differs by less than 0.01 from the proportions in 95 per cent. of the myriadfold sets in M. This is correct, but it is the merest restatement of my principle (1*b*). We must treat "p'" differently:

> (2*c*) For any value of p', 95 per cent. of the myriadic sample sets of composition p' which are included in a population M are such that the proportion of P in M differs from p' by less than 0.01.

This, so far as it is intelligible at all, is absurd, since for some values of p' the proportion in M must match all the sample sets while for the rest it can match none, and we don't know which is which until we have made our induction.

The traditional use of Bayes's theorem for establishing an inverse probability for an inference from an inductive sample to a specified population M does, oddly enough, treat M both as one and as many in a manner suggested by Mr. Kneale's phraseology. That is, it can be construed as considering the moot population, call it "M_j" this time, to be one of an array or sisterhood of populations, M_1, M_2, . . . M_{n+1}, all of the same size n, such that they respectively contain the predicate P in all the possible proportions from o/n to n/n. Our actual sample M_jQ, now, which consists of one set from M_j, has a known composition p'; and our inductive task is to discover, so to say,

which of the possible M-classes this M_1 is. Our reference class contains now just every sample set of the size and composition of M_1Q which is included in *any* of the M-classes, and the quaesitum character consists of 'coming from' (being included in) an M-class which has the composition $p = p' \pm x$ (where x is an appropriately small fractional margin). We can precisely calculate, differently for different p', what proportion of all the sample sets of the given size and composition p' come from populations with compositions in this neighbourhood, and hence the probability that our actual sample is one which came from a population in this neighbourhood, and hence the probability that the real population M_1 is in that neighbourhood and has such a composition. This is the procedure recently revived by Professor Carnap.[1] The inverse probabilities it provides are not quite the same as our corresponding direct ones, but they are of the same order of magnitude. And here, as with the direct rule, we can renounce specific probability calculations for every value of p', and say more simply and indeterminately that no matter what the sample may be like, most sample sets like it 'come from' populations like it (and in general that most of the sample sets are in populations which match them). If valid, it justifies the inductive inference in the framework of that theory of 'chances' which Mr. Kneale and I both accept. But we agree in rejecting it as invalid because its statistics are imaginary: the sisterhood of M-populations is an arbitrary fiction invented to make the probabilities come out right. Our own sample sets have been 'possible samples' to be sure, but they have been actual sets; and any inversion of my principle (1*a*) must require for its application a knowledge only of actual sample sets and their actual populations. Let us label the Bayes method "(2*d*)" for reference, however.

There suggests itself now that our inverse principle should mention other actual populations.

> (2*e*) 95 per cent. of the myriadic sample sets which are included in M and which have the composition p' are included in populations with compositions $p = p' \pm 0.01$.

But this is ambiguous in several ways. In particular, does the predicate clause mean *included in at least one population of that sort* or *included in no populations except of that sort?* The number of populations in which any sample set is included is definite

[1] Rudolf Carnap, *Logical Foundations of Probability*, Chicago, 1950, Appendix. More detail and defence of his theory of induction are promised in a second volume.

but stupendous: it equals the total number of selections of all sizes which can be made from k — s objects, where k is the number of things in the Universe and s is the number in the sample. To stipulate *at least one* matching population as above, then, is truistic but hopelessly ineffective; to stipulate *nothing but* matching populations is self-contradictory (granted that the sample MQ and the predicate class P do not nearly exhaust the Universe).

Many other such changes could still be rung on our formula (2*a*), but I cannot think of any which can plausibly be claimed to be both inversions of (1) and derivable from Mr. Kneale's (2). If we want a statistical principle plainly inverse to our original, which decreed a relative frequency within the class of sample sets included in any one population, we must quit talking merely about relative frequencies within other classes of *sample sets* and decree a relative frequency within the class of *populations* which include any given sample. Thus we might hope for the slightly indefinite law, applicable irrespective of the actual sample composition p':

> (2*f*) For every myriadic sample set S and for every class P, no matter what the proportion p' of Ps in S, at least 95 per cent. of the populations which include S must contain P in a proportion $p = p' \pm 0.01$.

Or, indeed, we might hope for some specific formula, like the one available for the direct inference, which would determine an exact probability for every value of p'. Only the most extravagant re-editing of Mr. Kneale's formula (2) can draw this rule out of it, but it is an obvious and unequivocal inversion of our original principle. It has some plausibility, and Mr. Kneale would doubtless agree with me that it would provide the same probability for an induction which I thought, and he denied, was provided by the original principle (1*b*). But it assuredly does not follow from the latter, and it is in fact false, as is especially evident if we suppose S to contain all the Ps in the Universe, whereupon S must be as unrepresentative as possible of all its populations.[1] Beyond this remark, I have found neither the patience nor the capacity to ascertain whatever complex and

[1] Without knowing anything about the Universe, we can tell from some pairs of samples that it is impossible for most of their populations to match each of them. Professor Max Black proposed the inversion (2*f*) for my (1*b*), and used this reflection to disprove it, in a review in *The Journal of Symbolic Logic*, Vol. XII, 1947, pp. 141-144. I spell "Universe" with a capital "U" to avoid confusion with the statisticians' use of "universe" for "population".

qualified law may govern this inverse relation, but I would not expect pure cosmic arithmetic to furnish anything significant in this direction. Let us remember, however, that in disproving that for every sample, in every respect, the great majority of its including populations must be statistically similar to it, we have done nothing to prove that, for every sample, in any respect, the great majority of populations are *dis*similar, nor to impugn our other proof that not merely most but all populations, not merely in most respects but in every respect, are statistically similar to most of their samples.

The argument so far is a bit anomalous. It runs that because the greatest diligence of both our parties hardly suffices even to formulate a usable inversion, valid or invalid, of the Bernoullian probability, it is *prima facie* unlikely that my party could surreptitiously or inadvertently have assumed such an inversion. It leaves room for the retort, then, that so much the worse: I have not so much as an apparent alternative to the invalid use of the original direct principle. But it puts us in a position to examine more perspicaciously whether there was anything wrong with the inductive use of the direct principle and whether there was occasion for inversion.

There is a way of formulating my central idea which has no 'direction' and does not raise the spectre of inversion at all. It is that among all *pairs* of classes such that one member of each pair is a population and the other an included sample set, the great majority are statistically homogeneous in any respect P, a 'statistically homogeneous' pair being one whose two members have approximately the same composition. Given any particular pair consisting of a population M and a sample MQ, then, it is highly probable that *it* is statistically homogeneous. This fact can be used quite indifferently to infer the statistical character of either term from that of the other; there is now no way of distinguishing a direct from an inverse inference.

Again, there are ways of converting the original formula which obviate the appearance of any later inversion. Instead of writing "95 per cent. of the sample sets in M are such that their compositions do not differ by more than 0.01 from the composition of M" we can write "95 per cent. of the sets in M are such that the composition of M does not differ by more than 0.01 from their compositions". Then if S is a sample set from M, there is a probability of at least 95 per cent. that it is such that the composition of M does not differ from its composition by more than 0.01.

Lest these seem like verbalistic dodges, however, let us start with the frankly 'direct', if somewhat indefinite, principle (1*b*), and lay out our procedure link by link.

Principle (1*b*) is an analytic truth, purely formal too in that it contains nothing but variables and logical constants. Now we consider a specific induction. Let M be the American electorate and P the property (or class) which consists of approving Mr. Polk for president. Simple ('deductive') substitution in the formula gives us that, no matter what the proportion p of Polkians among American voters, at least 95 per cent. of the 10,000-membered sets of American voters must contain Polkians in a proportion equal to $p \pm 0.01$. This is an analytic truth too, but a 'material' one in so far as it 'instantiates' the formal principle by supplying material constants for the variables "M" and "P" (though not for "p"). Let us call it our 'rule'. In Mr. Kneale's usage it means already that among the myriadic sample sets included in the American population, there is a 'probability' of at least 95 per cent. of a proportion of Polkians which differs from the proportion in the population (whatever that may be) by no more than 0.01. I should prefer to talk about 'probability' only in connexion with propositions, not properties, a little later. For we next select an actual sample, or turn our attention to a sample which has been waiting round. On the basis of observation, we thus conjoin with the foregoing 'rule' the following 'case', "This is in fact a myriadic sample of American voters". In relation to the conjunction of this rule and case, there is then a probability of at least 95 per cent. for the 'result' (call it "r"), "This sample contains Polkians in a proportion $p \pm 0.01$", where p, of course, is the (still unspecified) proportion of Polkians in the electorate. At this juncture, our one and only *probability* inference is over and done with. We have a high probability for the proposition r, in relation to the relevant and available evidence, and now can ignore the context for a moment and operate on r by the mechanisms of simple deduction. We can re-introduce "p'", *e.g.* to stand for the proportion of Polkians in the sample, and then rewrite r as follows: "$p' = p \pm 0.01$". By simple arithmetic, again, we can rewrite this: "$p = p' \pm 0.01$". Or again, "p is between $p' - 0.01$ and $p' + 0.01$"; that is, "The proportion of Polkians in the electorate lies between a figure 0.01 less than the proportion in this sample and a figure 0.01 more than it".

Now we poll the sample, or reflect on a poll already made, and find, let us say, that the proportion of Polkians in it is in

fact 0.43. We can supply this value for "p'" in any of the above equivalent expressions, in any case merely putting equals for equals. The most congenial results are perhaps the following, whose originals are easily identified: "$p = 0.43 \pm 0.01$", or "The proportion of Polkians in the American electorate is between 0.42 and 0.44". We can now recall the probability context, if we like: In relation to the premises at hand, there is a probability of at least 95 per cent. that the relative number of Polkians among the American electorate is between 0.42 and 0.44.

Now where, in all this, is there an illicit probability inversion, or *any* probability inversion? If there is an inversion, I submit, it must occur somewhere; and contrapositively, if it occurs nowhere, there is none. The phrase which seemed to Mr. Kneale to signalize an inversion was my loose verbal expression of the exchange of "$p' = p \pm 0.01$" for "$p = p' \pm 0.01$"; that is, the exchange of "The proportion in the sample is the same as the proportion in the population, give or take 0.01" for "The proportion in the population is the same as the proportion in the sample, give or take 0.01". But this is an impeccable deductive conversion *within* the probability conclusion r and not an inversion of the probability relation nor a transposition of probability reference classes. Being a wholly tautologous swap, it was dispensable anyhow, with only verbal detriment to our conclusion. The conversion of r, in contrast to an inversion of the probability relation, is warranted by a principle of probability which has a place, so far as I know, in every theory or calculus: that all propositions deductively equivalent to one another must have exactly the same probability in relation to any body of evidence. Mr. Kneale's school have a little special difficulty in acknowledging this principle because they speak of the 'probability' of properties, but I am sure that they accept it in effect. More generally, every day's intelligent living calls for the deductive manipulation of probability conclusions, in abstraction from their probability contexts, only tempering our belief in the results by the memory of how strong a cord of probability connects them with the evidence (and making sure that our operations do not adduce further and countervailing evidence). If we could not treat probable propositions by deductive procedures in this way, there would be virtually no use either for probable propositions or for deductive procedures. A glance back at the use of Bayes's theorem for inductive inference will see that even an inverse principle would not obviate such transformations.

I requoted from Mr. Kneale my analogy of the lottery, where the numbered tickets correspond to the sample sets with their compositions, and where in knowing that most of the ticket numbers are nearly equal to p, we *ipso facto* know that p is nearly equal to most of the ticket numbers. The person who objects to the direct probability of induction must hold either that a lottery of that sort would not provide a probability that a given ticket will reveal the right answer, or that the lottery differs from the inductive situation in some essential respect, or both. It would have been helpful if Mr. Kneale had explained which course he adopted, but I think that either would be hard to defend. Suppose, for another and somewhat closer analogy, that I have not met Alderman Murphy but am told infallibly that at least 9 of his 10 grown children are as tall as he, plus or minus half an inch. One of them is coming down the street; I anticipate with a probability of at least 0.9 that this child will be just about as tall as he, and I predict with the same probability that Alderman Murphy is just about as tall as this child. As the child passes, I size him up, and estimate then, with a probability of at least 0.9 and an error of at most 0.5 inch, what is Murphy's height. I do not need to know, for instance, what proportion of aldermen are of this child's height, or have children of this height; nor what proportion of children of this height have Mr. Murphy for father; nor what proportion of persons who are the same height as their fathers were begotten by Mr. Murphy, etc., etc.

The impression that there must be a probability inversion somewhere along the line of our argument is due partly to the mere tradition of inverse probability, but both it and the tradition are due mostly to the fact that we so easily formulate the inductive implication in ways which are verbally inverse to the formula for the inference from population to sample. "Given that the composition of the population is 0.43, what is the composition of the sample?" seems related to "Given that the composition of the sample is 0.43, what is the composition of the population?" as "Given that a man is a Democrat, what is his religion?" is related to "Given that he is a Presbyterian, what is his politics?" "The probability that a sample will match its population" and "the probability that a population will match its sample" seem, especially in Mr. Kneale's system, to be related like "The probability that a New Yorker will be a Reserve Officer" and "the probability that a Reserve Officer will be a New Yorker". They strongly suggest some interchange of premise and conclusion, or of reference class and

quaesitum class, with a consequent inversion of the probability relation. But we have found this appearance was delusory: that it is hard to formulate any such inversion at all, and that none is valid, but that this is strictly irrelevant because the one 'direct' law suffices for the inductive probability.

Two happy logical circumstances, resolutely neglected in most of the literature, are mainly responsible for this result: that the antecedent and consequent of an inductive probability turn upon the relation of statistical similarity, and that statistical similarity is a symmetrical relation. Mr. Kneale is of course right that the proportion of Ps among the Ms is not in general equal to the proportion of Ms among the Ps. Knowing the probability of an M's being P does not provide the probability of a P's being M. But special effects, which to some extent circumvent this difficulty, ensue when the conclusion states a relation, as in *Jones admires Mason*. In the first place, a (dyadic) relational statement has always a logically equivalent converse, as *Jones admires Mason* (aRb) is equivalent to *Mason is admired by Jones* ($b\check{R}a$). In the second place, a relation and its relatum may be lumped as what Professor Moore has called a 'relational property', like admiring-Mason (R"b) or being-admired-by-Jones ($\check{R}$"a). A relational conclusion, aRb, like *Jones admires Mason*, is equivalent therefore to each of two subject-predicate propositions, *Jones is admirer-of-Mason* (a *is* R"b) and *Mason is admired-by-Jones* (b *is* $\check{R}$"a). With respect to probability, then, it has a lien on two sets of reference classes. We can assign a probability to *Jones admires Mason* (aRb) if we either know the relative frequency of admiring-Mason (R"b) within some class of which we know Jones (a) to be a member, *or* know the relative frequency of being-admired-by-Jones ($\check{R}$"a) within some class of which we know Mason (b) to be a member. It would *not* in general suffice to know the relative frequency of R"a within a b-class nor of $\check{R}$"b within an a-class—*e.g.* to know the relative number of persons like Mason (in some given respect) who admire Jones, or the number like Jones who are admired by Mason. But the situation is relaxed again if the relation R is symmetrical, that is, equivalent to its converse, as *accompanying* is equivalent to *accompanied-by*. To assign a probability to *Jones accompanies Mason* we have, so to speak, a quadruple opportunity: it is enough to know the relative frequency of either accompanying-Mason (R"b) or being-accompanied-by-Mason ($\check{R}$"b) in a Jones-class, or of either being-accompanied-by-Jones ($\check{R}$"a) or accompanying-Jones (R"a) in a Mason-class (which may be or not be identical with the Jones-class).

Now the conclusion of our inductive probability *is* a relational proposition: *This sample matches that population within the margin 0.01* is hence equivalent to its relational converse, *That population is matched by this sample.* . . . And because its relation moreover is symmetrical, it is convertible with *That population matches this sample within the margin 0.01* (and so with *This sample is matched by that population* . . .). We may hence derive an impeccable probability for any of these four equivalent propositions from either a knowledge of the relative frequency of matching or being matched by that population within a class of samples to which this sample belongs, or a knowledge of the relative frequency of matching or being matched by this sample within a class of populations to which that population belongs. *Any* class which met any of these requirements would do. Concerning one appropriate class of samples, and indeed the one which most naturally comes to mind, namely, the class of sample sets of the given size which are included in that population, we do know, *a priori*, not the exact frequency of closely matched and matching sets, but an absolute minimum which is still great enough for our inductive purpose: specifically, *e.g.* that at least 95 per cent. of the myriadic sample sets match (and are matched by) that population within 0.01. Given this, we have no use whatever for any other or 'inverse' frequency. Yet we can still look for one if we like—the most plausible question being, we have noticed, "What is the relative frequency of closely matching *populations* among all the populations which include this sample?" That this question, and any of the many analogous questions about relevant classes of classes in the inductive situation, appear to have no ascertainable answer, far from being a blow to the inductive philosopher, should be a relief. He might have taken extra comfort, to be sure, if it could have been demonstrated that the relative frequency of matching populations was as great as the relative frequency of matching samples; but he would have been decidedly embarrassed if it had been demonstrably very much less. For just because the double-endedness of relational conclusions gives occasion for extra probability determinations, it can give occasion for conflicting determinations. If $m/n\ M$ is R"b, and a is an M, but $m'/n'\ (\neq m/n)\ S$ is $\breve{R}$"a, and b is S, what is the 'net' probability of aRb? If, for example, $\frac{3}{4}$ of the men in Smith's platoon are taller than Perkins and $\frac{1}{3}$ of those in Perkins's platoon are shorter than Smith, what is the probability that Smith is taller than Perkins? This puzzle subsists whether or not the reference classes (the platoons for example) are the

same, and is perhaps one degree more awkward than the standard 'cumulative' puzzle over plural reference classes such as arises when we know simply that a is M and $\frac{3}{4}M$ is P, but that a is also S and $\frac{1}{3}S$ is P, and ask what is the net probability that a is P. The twin puzzles affect all theories of probability alike, in every area, and there is nowhere any accepted solution for them. At worst, the present version would be no special liability of our theory, nor of the inductive area; but in fact, as we have found, it does not affect us at all, just because there is no reference class other than our class of sample sets which touches the inductive inference and for which a contrary frequency is demonstrable.[1]

We have so far been examining the justification for the relational conversion of our primary proposition *r*, *This sample closely matches that population*, or "$p' = p \pm 0.01$", to *That population closely matches this sample*, or "$p = p' \pm 0.01$". Since the convertend and converse are here logically equivalent, no exception can be taken to this substitution without abandoning the best established part of probability theory. Somewhat different however is our final substitution, above, when we put "0.43" for "p'" (or for "the proportion in this sample"). The candid newcomer, to be sure, might think it perverse to suspect that while we can validly carry along our minimum probability for the sample's matching the population, irrespective both of p and of p', so long as it is put to no inductive use, the moment we try to cash in on it by taking a look at p' in the sample, pouf! the probability goes glimmering. Yet there is some real excuse for the invidious distinction. For "$p' = 0.43$," though we imagine it virtually certain as a result of our poll, is an empirical and not a logical truth. It is not like *Nine is the square of three*, but like *Nine is the number of the planets*, and the original proposition and the new one achieved by this substitution are not logically but only materially equivalent. Such substitutions are notoriously not generally valid in modal

[1] Professor Black, *loc. cit.* p. 143, ingeniously developed the possibility of paradox from these sources. Doubtless unintentionally, he gave the impression that the facts (1) that the direct and inverse frequencies might conflict and (2) that only the direct frequency is discoverable, constituted *two* faults in my doctrine, whereas actually they cancel each other and constitute no fault. Persons not inured to the philosophy of probability may feel uncomfortable with the principle that what we don't know to affect the probability can't affect it. But this is the condition of there being any *probability* at all. In relation to the sum total of facts which would be relevant if we knew them, every proposition is certainly true or certainly false.

contexts, including probability contexts. If we have a probability of 65 per cent. that the Man in the Iron Mask was the Duc de l'Eglise, for example, and then establish with virtual certainty that the Man in the Iron Mask was Jonathan Edwards, we cannot declare a probability of 65 per cent. that Jonathan Edwards was the Duc de l'Eglise. For the new proposition that the Man in the Iron Mask was Jonathan Edwards (in conjunction with certain standard items of information) constitutes new evidence relevant to *The Man in the Iron Mask was the Duc de l'Eglise*, greatly diminishing its probability. It has been argued that the proposition that p' is in fact 0.43 is similarly relevant—as relevant as the size of the sample, for example, of which we did take account—so that it is illicit to keep for our conclusion merely the generic probability, 95 per cent. in our examples, which pertains to the whole class of myriadic sample sets in contradistinction to the subclass of those which evince the proportion 0.43. Followers of Carnap make this objection, and Professor Black has broached it (*loc. cit.*). Mr. Kneale tacitly makes it in proposing 'inversions' that stipulate at once the real and exact value of p' (for example, 0.43), which is indeed the most conspicuous component of any actual induction and looks like its only determinate and material evidence. Yet that this apparently essential factor is irrelevant to the inductive *probability*, and never provides part of the evidence in relation to which the latter is determined, is just the lesson of the failure of the inverse method, and the main burden of my argument. In order that it should be relevant, some *other* probabilistic solution of the problem of induction must be already at hand. Specifically, there must be in evidence, logically or materially, what is the relative frequency of matching among samples of this composition, in distinction from samples of all compositions together, and that this frequency is *different* from the frequency among the larger class. We have found that this is not in evidence. So long as we confine ourselves to *a priori* knowledge of actual sets, which is all that is pertinent for our purpose, we know just nothing about any such special proportion. We do know, of course, that the relative frequency, among the sample sets of M having the composition 0.43, of sets which match the population is in fact either 0 or 1, but this is just the same as knowing that either this sample matches or it doesn't, and no more affects the current probability that the sample is one which matches than the abstract reflection that John Johnson must in fact either die or not die of tuberculosis this year discredits or supersedes the best available

probability for this event, say 0.002, which is provided by actuarial data.

The classical method of inversion now being rehabilitated by Carnap does lay down a special frequency for every sample composition, so that the latter is correspondingly relevant in Carnap's system. But this presupposes, of course, his inductive probabilities extracted from the census of purely imaginary populations and sample sets which we have considered illicit. I do not wish to preclude the possibility that a better method than his very complex one, or my very simple one, may achieve an analogously specific result by counting real sets. I can contemplate all such developments with a quiet mind because, if they prove fruitful, they cannot in general *diminish* the inductive probability which I allege. In the direct and purely deductive use of the Bernoullian results, the knowledge of the composition p of the population can make none but a favourable difference in comparison with our minimum figure which ignores p; but our minimum figure is so great that there is not *room* for a difference of any consequence. Carnap's inverse method surprises us a little by determining that, given p' in the sample, the most probable value of p for the population is not generally quite equal to p'; but the difference is slight and covered by our harmless rim of indeterminateness. On no scheme can the stipulation of specific values be generally unfavourable, and it is not easy to conceive any way in which some special sample compositions might have 'an insignificant chance'[1] of matching their populations. Speaking loosely, for the suggestion is loose and obscure, we can say that, since it is indisputable that all populations are matched by nearly all their samples, counting all compositions together, it follows that if, *per impossibile*, sample sets of certain compositions could be proved to match with a relative frequency much less than, say, our 95 per cent., the sample sets of other compositions would thereby be proved to match with a relative frequency much more. If, on the other hand, all such efforts to refine the inductive probability remain futile, then the exact composition of the sample remains irrelevant to it and my blunt evaluation of it must prevail.

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[1] Black, *loc. cit.*

IV.—JEVONS AND LOGIC

By W. Mays and D. P. Henry

Abbreviations :

PL. *Pure Logic* (E. Stanford, 1864).
SS. *Substitution of Similars* (Macmillan, 1869).
PS. *Principles of Science*, 2nd. edn. (Macmillan, 1887).
SE. *Studies and Exercises in Deductive Logic*, 2nd. edn. (Macmillan, 1884).

There is a need for a revaluation of the logical work of Jevons, especially as he was a pioneer in the mechanisation of logic. His achievements in this direction have been overlooked and remain relatively unknown. Jevons seriously believed that he was the discoverer of a new kind of logic, and records in his Journal[1] an illumination resembling that of Descartes when he discovered co-ordinate geometry. He tells us : " As I awoke in the morning, the sun was shining brightly into my room. There was a consciousness on my mind that I was the discoverer of the true logic of the future. For a few minutes I felt a delight such as one can seldom hope to feel." If Jevons were alive today it is unlikely that he would be surprised by modern digital computors and the arithmetical marvels which they perform.

The new type of logic he had in mind, which he calls " combinational logic " is better known today under the name of the truth-table method. Jevons was, however, much more concerned with its use as a device for drawing inferences than as a method for testing formulæ, which seems to be its main function in modern text-books, though as a method of inference it has certain limitations not foreseen by Jevons : for example, it is strictly applicable only to the calculus of propositions, and not to the predicate calculus.

Why, then, has Jevons' work been neglected over the last four decades ? This to some extent is due to logic taking a rather different path from that which he anticipated. Unlike Peirce and Frege, he paid little attention to the logic of relations and quantification theory. In his enthusiasm for putting over an important idea, he did not concern himself with the other wider aspects of the subject. This was one of the reasons for Jevons' work failing to influence logicians of the Russell-Whitehead school : their interest lay in establishing number

[1] *The Letters and Journal of W. Stanley Jevons*, edited by his wife (Macmillan, 1886), p. 219 (entry for 28*th March*, 1866).

upon a firm logical basis. They developed accordingly a special symbolism for relations and quantifiers, which led to the development of the predicate calculus.

Though Jevons might be said to have overlooked the main lines of enquiry of modern logic, he had, nevertheless, in his method of indirect inference hit upon an idea of some theoretical and practical importance. One of the reasons for its neglect was the apparent tediousness and lack of general rules. The method appeared rather barbarous to logicians, proceeding as it did by the mechanical elimination of incompatible alternatives, and not from the direct intuition that one premise naturally follows from another. Now that we are familiar with the method of truth-tables this objection has little force.

Jevons' logical system was regarded by him as being to a large extent founded on the work of Boole. "The forms of my system", he says (PL., p. 3) "may in fact be reached by divesting his (*i.e.* Boole's) system of a mathematical dress, which, to say the least, is not essential to it." His system, however, differed from that of Boole in being a logic of terms. He also regarded all propositions as being equational in form, and the principle which became the mainspring of his system was the "Rule of Substitution": "For any term substitute what is stated in any premise to be identical in meaning with that term." (SE., p. 181). Though the logic of Jevons is essentially a logic of terms, he was profoundly dissatisfied with this concept; he used it however, since he did not have at his disposal a better instrument. "But the continued study of logic convinces me that this doctrine of terms is really a composite and for the most part an extra-logical doctrine" (SE., p. 1). He thus excuses himself for turning to the discussion of propositions after dealing perfunctorily with terms. By a term he means the quality or group of qualities which belong to or mark out a class of objects. In his logic, however, terms are used rather as variables; "blank letters devoid of special signification, such as A, B, C, etc." are taken as representing any noun. "Just as in Algebra $x, y, z, p, q,$ etc. are used for *any quantities* undetermined or unknown . . . so will our letters stand for undetermined or unknown things" (PS., p. 13), *i.e.* the range of possible objects which these variables may be said to designate. A proposition is defined by him as "a statement of sameness or difference of two terms" (PL., p. 8). Jevons is not concerned with the specific meaning of these terms nor with the empirical truth or falsity of the resultant propositions, but with the truth or falsity (*i.e.* consistency or inconsistency) of one proposition as depending upon others. And this

links up with his use of the Logical Alphabet as a method of testing the truth of a proposition.

It is difficult at first to discover what Jevons means by an identity of meaning upon which his equational logic rests. It might appear that he was referring to an identity of qualities connoted by the two terms. The 'substitution of similars' would then consist in substituting terms having similar connotations for one another. Jevons however, is merely interested in terms as representing classes of things. Two names are then identical (or substitutable) when they designate similar things. Consider Exogens = Dicotyledons; here the group denoted by one term is identical with that denoted by the other in everything except name. They are symbols for the same group of objects; the equivalence sign then refers to such an identity (*cf.* 'Mr. Jevons' Formal Logic', G. C. Robertson, MIND 1876, pp. 213-214). He gives an example of this. "There are millions of cells in a human body, but if each cell of one person were represented by an exactly similar cell similarly placed in another body, the two persons would be undistinguishable, and would be only *numerically* different" (PS., p. 19). Obviously this is another statement of Leibniz's identity of indiscernibles and the thesis of extensionality.

Jevons' reason for taking identity as a fundamental relation rather than class inclusion stems from his account of classes in terms of individuals. Class inclusion he believes can be defined in terms of the correlation of one group of individuals with part of another group. Thus when we state mammals are vertebrates, mammals can only be included in vertebrates if they are identical with part of the class of vertebrates, or expressing it symbolically A = AB, the predicate denoting exactly the same individuals as the subject A (mammals), *i.e.* the individuals which are vertebrates and mammals. Jevons refuses to base his logic on class-inclusion since for him classes only exist as groups of individuals, and class inclusion is just another instance of identity: an identity between the whole of one group of individuals and part of another.

Jevons' Symbolism and the Logical Alphabet

Jevons' symbolism and its rules of manipulation are described by him as follows (SE., Chap. XXI, *Elements of Equational Logic*). In each case the propositional analogues in modified *Principia Mathematica* notation are also shown in parentheses.

"A, B, C, or other large capital letters signify qualities, or groups of qualities" (a, b, c, etc.).

"a, b, c, or other small . . . letters are the corresponding negative terms" ($\sim$a, $\sim$b, $\sim$c, etc.).

"The mark = is the sign of Identity of Meaning of the terms between which it stands" ($\equiv$):

"The sign $\cdot|\cdot$ signifies *unexclusive alternation*" ($\mathbf{v}$).

"Juxtaposition of two letters forms a term whose meaning is the sum of the qualities signified by the two letters, thus AB means a union of the qualities of A and B" (a . b).

"The Laws of Combination of these symbols are as follows:—

The Law of Commutation

$AB = BA$ [a . b $\equiv$ b . a]

The Law of Simplicity

$AA = A$ [a . a $\equiv$ a]

The Law of Unity

$A \cdot|\cdot A = A$ [a $\mathbf{v}$ a $\equiv$ a]

The Law of Distribution

$A(B \cdot|\cdot C) = AB \cdot|\cdot AC$ [a . (b $\mathbf{v}$ c) = a . b $\mathbf{v}$ a . c]

The Law of Indifferent Order

$B \cdot|\cdot C = C \cdot|\cdot B$" [b $\mathbf{v}$ c = c $\mathbf{v}$ b]

The 'Laws of Thought' are given in the same work (SE., p. 180) thus:—

The Law of Identity

$A = A$ [a $\equiv$ a]

The Law of Duality or of Excluded Middle

$A = AB \cdot|\cdot Ab$ [a $\equiv$ a . b $\mathbf{v}$ a . $\sim$b]

The Law of Contradiction

$Aa = 0$ [$\sim$(a . $\sim$ a)]

The word "combination" is used by Jevons to refer to the analogue of the "and" operation of the propositional calculus; (*cf.* "AB" above). The structural resemblance of Jevons' '=' to the '$\equiv$' of the propositional calculus is illustrated in many contexts, and corresponds to reciprocal implication, *e.g.* (PL., p. 9). "From A = B, so far as we know B, . . . we can learn A; so far as we know A, we can learn B." In the course of his discussion of Boole's system (PL., p. 75 es.) he clarifies his assertion that unexclusive disjunction is the most primitive meaning of 'or'. "It seems likely, indeed, that this distinction of exclusive and unexclusive alternatives is the Gordian knot in which all the abstract logical sciences meet and are entangled" (PL., p. 84).

Jevons' "Law of Duality" (Law of Excluded Middle) forms the basis for the development of his system of Indirect Inference which was the condition *sine qua non* for the invention of his logical machine.

"The successive application", he says (SE., p. 180) "of the Law of Duality to two, three, four, five or more terms, gives rise to the development of all possible logical combinations, called the Logical Alphabet, the first few columns of which are given below." The specimens of the Alphabet given here, and in other contexts (e.s. PS., p. 94; PL., p. 45; SS., p. 54) include:

AB	ABC	ABCD	ABCDE	ABCDEF
Ab	ABc	ABCd	ABCDe	ABCDEf
aB	AbC	ABcD	ABCdE	ABCDeF
ab	...	....	.....	.....
	abc	abcd	abcde	abcdef

It is evident that this alphabet exhausts all the alternative affirmative or negative combinations of a given number of terms. The logical machine, as will be seen later, was a mechanical device for selecting those combinations of the Alphabet compatible with any proposition or propositions. In addition to this mechanical way of indicating selections, Jevons uses various other modes of exhibiting such compatible combinations. In the *Principles of Science* (p. 135), '1' and '0' are used for denoting selected and rejected rows respectively. Indeed he appears at one point in his note-books (in the possession of the Department of Philosophy, Manchester University), to have considered the possibility of representing the Alphabet as a whole in terms of these two symbols. Dots and spaces on squared paper are also used in the note-books to denote selections and rejections. The most important example of the use of such selections (apart from the machine) occurs in SE., Ch. XXVII, where all the possible selections from an Alphabet for three terms (256 in all) are listed, together with the propositions to which they correspond. Here $\alpha\,\beta\,\gamma\,\ldots\,\theta$ are used to denote each of the eight rows of the three-term Alphabet, in order to facilitate reference to various sets of selections. Examples nos. 1 to 6 of this "Logical Index", together with their propositional analogues are as shown in table on opposite page. In the cases listed in the table $\alpha, \beta, \gamma,$ is added on to the last column, thus 6 is read (α, β, γ, δ, ϵ, , η, .). In binary notation this becomes (1 1 1 1 1 0 1 0). The kinds of equivalence holding between the terms are classified under Type numbers (PS., p. 140 and p. 143).

Jevons tells us that a corresponding index for four terms will occupy one volume of 1024 pages, and that an index for five terms will fill a library of 65,536 volumes each containing 1024 large

Number	*Type*	*Present, α, β, γ*	*Present*
1		All present	δ ϵ ζ η θ
2	VIII	ab = abC (∼a . ∼b) ⊃ c	δ ϵ ζ η
3	VIII	ab = abc (∼ a . ∼ b) ⊃ ∼ c	δ ϵ ζ θ
4	II	a = aB ∼a ⊃ b	δ ϵ ζ
5	VIII	aB = aBC (∼a . b) ⊃ c	δ ϵ η θ
6	II	a = aC (∼a ⊃ c)	δ ϵ η

pages. However, with the development of electrical storage devices, the amount of space required for such an index can be very considerably reduced. The following table (PS., p. 142) shows the astronomical increase in the number of possible logical relations with an increase in the number of terms.

Number of terms	*Number of possible combinations*	*Number of possible selections of combinations corresponding to consistent or inconsistent logical relations*
2	4	16
3	8	256
4	16	65,536
5	32	4,294,967,296
6	64	18,446,744,073,709,551,616

The Logical Alphabet for any number of terms consists of the 2^n terms which in Boole's system form the expansion of '1'. Jevons stresses its importance for logic and tells us that there is a close connexion between what he calls the Arithmetical Triangle (Table of binomial coefficients) and the Logical Alphabet. He believes that the theory of combinations lies at the base of empirical science and mathematics. Nature too has a combinatorial character; "Nature to us is like an infinite ballot box, the contents of which are continually drawn, ball after ball and exhibited to us. Science is but the careful observation of the succession in which balls of various characters present themselves" (PS., p. 150). The set of combinations making up

the Logical Alphabet may be taken as typifying the abstract structure of the world. There is an isomorphism between the assumed combinatorial character of the universe and the Logical Alphabet.

The whole point of the " substitution of similars " was missed by Jevons' contemporaries ; they overlooked the fact that as far as his logic was concerned it was not merely an identity of names, but an identity against the background of the Logical Alphabet. For Jevons two propositions are identical, and may be substituted one for the other, if they remove the same number of combinations from the Logical Alphabet (*i.e.* have the same normal disjunctive form). He thereby gives what in effect is a definition of material equivalence. Material implication is defined on similar lines. " A proposition is inferable but not equivalent to another when it removes some but not all the combinations which the other removes " (PS., p. 116). For example, if we take two propositions " p " and " q ", and if " p " has in its expansion a rejected combination (o) where " q " has a selected combination (l), and if every rejected combination (o) of " q " is also a rejected combination (o) of " p ", then " q " is inferable from " p ". This is a round about way of saying, that " o ⊃ l " and " o ⊃ o ".

The Indirect Method

The difference between the ' Indirect ' and ' Direct ' methods of inference, Jevons tells us (PS., p. 81), is exactly analogous to that between the direct and indirect methods of proof in geometry: " Euclid often shows that two lines are equal, by showing that they cannot be unequal, and the proof rests upon the known number of alternatives, greater, equal or less, which are alone conceivable." Logically, the problem is to discover which of the alternative combinations of the Logical Alphabet are compatible with a given premise or premises. In his discussion of the method, however, he often expresses himself obscurely, and the obsolete symbolism and terminology employed hides much of what he has to say from the modern reader. The following account of the principles underlying Jevons' method is therefore not given in terms of his " Class Logic " and its symbolism, but we use instead its analogues in the propositional calculus, and a modified *P.M.* notation. For example, the premises with which he dealt consisted of two terms united by the " = " sign, corresponding to the copula of the proposition. The terms thus united could be simple or complex (*i.e.* could consist of alternations or conjunctions of terms). A

proposition such as " All A is B " was expressed as " A = AB ", which, when transformed into its propositional analogue (" a ≡ a . b ") may be seen to be equivalent to " a ⊃ b ". To express " All A is B, and conversely, all B is A " he used " A = B " (*i.e.* " a ≡ b ").

Suppose, therefore, " p ⊃ q " to be the propositional analogue of a typical premise. Using " o " to indicate a contradictory conjunction of propositions, it will be found that the logical multiplication of all the alternatives of the " Logical Alphabet " by this premise gives the results shown on the right-hand side of the following equation.

$$(p \,.\, q \vee p \,.\, \sim q \vee \sim p \,.\, q \vee \sim p \,.\, \sim q) \,.\, (p \supset q) \equiv (p \,.\, q \vee (o) \vee \sim p \,.\, q \vee \sim p \,.\, \sim q) \qquad (1)$$

i.e. the non-contradictory combinations of the right-hand side of (1) constitute the required selection from the total alternatives of the Logical Alphabet, which on the left-hand side are multiplied by the premise.

Jevons grasped this principle, and outlined the following procedures for obtaining such consistent combinations (P.L., pp. 44-46) :—

- (α) " *Development.* Any premises being given . . . form all the possible combinations of the simple terms."
- (β) " *Comparison.* Combine successively each such combination with both members of a premise." (This corresponds to the left-hand side of (1) above.)
- (γ) Eliminate the products thus arrived at which violate the Principle of Contradiction. The remaining combinations are the consistent ones required (*cf.* right-hand side of (1) above).

The criteria for the eliminations mentioned in (γ) arise simply from the equational form in which Jevons set his premises. Those combinations which as a result of procedure (β) produce a contradiction on one side or another (but not both) of the equationally expressed product are eliminated. The remainder are selected as consistent with the premise.

Jevons summarises the process in a table (PL., p. 46). The table we give below differs however from the original in its headings and in being couched in terms of the propositional analogues of Jevons' symbols. The premise taken is " A = BC " (*i.e.* " a ≡ b . c "). The columns are marked α, β, γ to correspond with the above procedures.

(α) *Series of alternatives*	(β) *Product of alternatives and premise* "a≡b . c"	(γ) *Selection or rejection of corresponding alternatives*
a . b . c	a . b . c≡a . b . c	Selected
a . b . ∼c	a . b . ∼c≡a . b . c . ∼c≡o	Rejected
a . ∼b . c	a . ∼b . c≡a . b . ∼b . c≡o	Rejected
a . ∼b . ∼c	a . ∼b . ∼c≡a . b . ∼b . c . ∼c≡o	Rejected
∼a . b . c	o≡a . ∼a . b . c ≡∼a . b . c	Rejected
∼a . b . ∼c	o≡a . ∼a . b . ∼c≡∼a . b . c . ∼c ≡o	Selected
∼a . ∼b . c	o≡a . ∼a . ∼b . c≡∼a . b . ∼b . c≡o	Selected
∼a . ∼b . ∼c	o≡a . ∼a . ∼b . ∼c≡∼a . b . ∼b . c . ∼c≡o	Selected

The general principles outlined above will now be applied to the particular case considered by Jevons in relation to his machine. The continued use of propositional analogues will enable us to summarise more concisely the rather complex account of the further logical principles involved in his " On the Mechanical Performance of Logical Inference " (*Philosophical Transactions of the Royal Society*, 1870, pp. 497-518).

Using the same premise (*i.e.* " p ⊃ q ") and transposing the bracketed expressions of the left-hand side of (1), the following equivalence may be set up:

$$(p \supset q) \equiv (p \supset q) \,.\, (p \,.\, q \vee p \,.\, {\sim}q \quad \vee {\sim}p \,.\, q \quad \vee {\sim}p \,.\, {\sim}\, q) \qquad (2)$$

Application of the equivalence:

$$p \,.\, (q \vee r) \equiv (p \,.\, q \vee p \,.\, r)$$ Jevons' " Law of Distribution ")

to the right hand side of (2) gives:

$$(p \supset q) \equiv [(p \supset q) \,.\, (p \,.\, q \vee p \,.\, {\sim}\, q)] \vee [(p \supset q) \,.\, ({\sim}p \,.\, q \vee {\sim}p \,.\, {\sim}\, q)] \qquad (3)$$

This leads to the same result as before, but represents more faithfully the problem as it presents itself for mechanisation. The machine, in effect, is required to multiply out the right-hand side of (3). It will be noted that in this case, where the premise is of the form " p ⊃ q ", the " ∼p " alternatives in the last square bracket merely reproduce themselves on multiplication; hence they may be expressed without actual multiplication. Jevons calls alternatives of this type " excluded " combinations. The term " excluded " is somewhat misleading, as they are returned to the end-result (having been stored during the working of the problem), after the set of alternatives affirming " p " (in the

first square bracket) has been multiplied out. This applied only to operations on premises of the form " $p \supset q$ " (*e.g.* A = AB). When a premise of the form " $p \equiv q$ " (*e.g.* " A = B ") is being dealt with, the same criteria of " exclusion " do not apply. For example the operation :

$$(p \equiv q) . (p . q \vee p . \sim q \vee \sim p . q \vee \sim p . \sim q) \qquad (4)$$

will not leave unchanged all the ' $\sim p$ ' alternatives, for here " $(p \equiv q) . (\sim p . q)$ " produces a contradiction. As a result, such premises (which Jevons calls " simple identities ") have to be analysed into two separate components (" partial identities "). *e.g.* " A = B " $(p \equiv q)$ into " A = AB " $(p \supset q)$ and " B = BA " $(q \supset p)$. In dealing with the mechanical aspects of this (PS., pp. 111-112), Jevons says : " It is no doubt a remarkable fact that a simple identity cannot be impressed upon the machine except in the form of two partial identities, and this may be thought by some logicians to militate against the equational mode of representing propositions."

The Logical Machine

The work of Jevons' logical machine thus consists in multiplying the combinations of the logical alphabet by the premise imposed on it in a similar manner to that outlined above. Lest it be assumed that this analysis of the machine's mode of functioning is given because of its antiquarian interest alone, one needs to point out that an examination of his machine is of value in itself. Operating, as it did, by simple mechanical methods, it illustrates in a clear and direct way the general principles upon which logical machines, electrical or otherwise, are based. Most accounts of modern electrical logical machines involve reference to circuits and complicated wiring diagrams. However, not only is the design of Jevons' logical machine simple, but as a logician he was concerned to relate it to logical theory, and not merely to use it for working out problems. His account is hence instructive, as it gives a valuable insight into the logical principles which such machines exemplify.

There are several discrepancies between the mode of operation of the machine as described in the *Principles of Science* (pp. 109-112), and that given in the Royal Society paper. However these make no difference to the outcome. The end-results are exhibited on two faces which are replicas of each other ; only one face (that above the keyboard) will be considered here. Each face consists of a series of vertical rods capable of independent vertical movement. In (5) the letters of the Logical

Alphabet are imprinted on the rods, and the symbols (*not* used by Jevons) by which they will be designated, $A_1\,A_2$ . . . etc., are shown.

Rod Designation :	A_1	A_2	A_3	A_4	A_5	A_6	A_7	A_8	a_1	a_2	a_3	a_4	a_5	a_6	a_7	a_8
	A	A	A	A	A	A	A	A	a	a	a	a	a	a	a	a
Rod Lettering :	B	B	B	B	b	b	b	b	B	B	B	B	b	b	b	b
	C	C	c	c	C	C	c	c	C	C	c	c	C	C	c	c
	D	d	D	d	D	d	D	d	D	d	D	d	D	d	D	d

(5)

The rods define the primitive symbols of the system : the positions into which they may be thrown refer to the logical operations imposed upon them. Hence, as Jevons points out in *On the Mechanical Performance of Inference* (p. 517) ; "The machine is thus the embodiment of a true symbolic method or Calculus." Also (*ibid.*, p. 508) : "The task of constructing a mechanical logic is thus reduced to that of classifying a series of wooden rods representing the conceivable combinations of the abecedarium" (*i.e.* the Logical Alphabet) "into certain groups distinguished by their position".

On the reverse side of the rods he employs a pin-notation (given in his note-books as well as in the account presented to the Royal Society). If we, as it were, look through the letters on the front of the baywood rods to the pins in the rear which operate them, we obtain the binary coding of these letters, the patterns being varied in each rod to correspond with the letters of the Logical Alphabet on their face, thus :

.		.		.		.		a b c d
.		.		.			.	a b c D
.		.			.	.		a b C d
.		.			.		.	a b C D
.			.	.		.		a B c d
.			.	.			.	a B c D
.			.		.	.		a B C d
.			.		.		.	a B C D
	.	.		.		.		A b c d
	.	.		.			.	A b c D
	.	.			.	.		A b C d
	.	.			.		.	A b C D
	.		.	.		.		A B c d
	.		.	.			.	A B c D
	.		.		.	.		A B C d
	.		.		.		.	A B C D
A	a	B	b	C	c	D	d	

The vertical movement of the rods permits them to occupy four positions, in only one of which, however, the letters are visible. These movements may be shown as follows :

Possible positions of variable—letter	II I Vision slit III IV

It is apparent that the Ist position is that in which the rod-letters are visible.

These four positions of the rods correspond to the following logical possibilities :

I. Neutral or initial position in which combinations are visible. It contains combinations consistent with the premises, thus comprising (when the " Full Stop " key is operated), those combinations " excluded " in Jevons' special sense.

II. Position of, or store for, " excluded " combinations, which are brought back into position I when the " Full Stop " key is operated (unless meanwhile contradicted by the consequent).

III. Position of, or store for, combinations inconsistent with the premise as a whole ; also temporary position of combinations inconsistent with a particular variable of a complex hypothetical disjunctive, *e.g.* in " $a \supset b \vee c$ " ($A = AB \cdot | \cdot AC$) the " c " (C) combinations containing " $\sim b$ " (b) are stored here, and returned to position I when the whole premise has been " played ". (Variable symbols in italics indicate that mechanical processes or their outcome are under discussion.)

IV. Final position of combinations inconsistent with the premise(s) as a whole.

The piano-like keyboard of the machine contained sixteen letter keys (antecedents and consequents), corresponding to the rods ; operation keys (two for " disjunction " and one for the " copula " which, as we have seen, corresponded as far as the machine was concerned to " $\supset$ ") ; " Full Stop " key which returned to the Ist position the " excluded " combinations, and placed in the IVth position those inconsistent combinations temporarily stored in the IIIrd position ; and the " Finis " key, which erased the previous working, restoring all combinations to the Ist position.

The mechanical cycle of operations, together with the logical analogues visible on the face of the machine may now be exhibited. Mechanical states are prefixed M_1, M_2, etc. ; rod-positions are indicated by the bracketing of their designation with the position numeral placed as in (7) below. The logical

analogues will be expressed partly in terms of binary selections from the Alphabet shown at (5), and prefixed L_1, L_2, etc. They are also given in terms of the propositional calculus; in any case the machine can be used indifferently for expressing class or propositional relationships.

Logically, the combinations initially visible are:

$$(a * b * c * d) \equiv 1111111111111111 \qquad (6)$$

(Here * between variables refers to all the possible alternative combinations into which they can enter.)

Mechanically, the initial state of the machine is:

$$[A_1\ A_2\ A_3\ A_4\ A_5\ A_6\ A_7\ A_8\ a_1\ a_2\ a_3\ a_4\ a_5\ a_6\ a_7\ a_8]\ \mathrm{I} \qquad (7)$$

When the premises "$(a \supset b)$. $(b \supset c)$" (or A = AB and B = BC) are impressed upon the machine, the sequence of keys struck (indicated by ○→) is:

(Premise "$a \supset b$"): antecedent "a" ①^{a}→; "copula" ②→; consequent "b" ③$_{b}$→; Full Stop ④^{FS}→.

(Premise "$b \supset c$"): antecedent "b" ⑤$_{b}$→; "copula" ⑥→; consequent "c" ⑦$_{c}$→; Full Stop ⑧^{FS}→.

Premise "$a \supset b$"

① ^{a}→
L_1; $[(a * b * c * d) . a] \equiv 1111111100000000 \equiv a$ (8)
M_1; $[A_1\ A_2\ A_3\ A_4\ A_5\ A_6\ A_7\ A_8]$ I, $[a_1\ a_2\ a_3\ a_4\ a_5\ a_6\ a_7\ a_8]$ II

② →
L_2; [Rests at L_1].
M_2; [Rests at M_1]. (9)

③ $_{b}$→
L_3; $[(a * b * c * d) . (a . b)] \equiv 1111000000000000 \equiv (a . b)$ (10)
M_3; $[A_1\ A_2\ A_3\ A_4]$ I, $[A_5\ A_6\ A_7\ A_8]$ III, $[a_1\ a_2\ a_3\ a_4\ a_5\ a_6\ a_7\ a_8]$ II

④ ^{FS}→
L_4; $[(a * b * c * d)] . [(a . b) \vee (\sim a . b) \vee (\sim a . \sim b)] \equiv$
$1111000011111111 \equiv (a \supset b)$ (11)
M_4; $[A_1\ A_2\ A_3\ A_4]$ I, $[A_5\ A_6\ A_7\ A_8]$ IV, $[a_1\ a_2\ a_3\ a_4\ a_5\ a_6\ a_7\ a_8]$ I.

Premise "$b \supset c$"

⑤ $_{b}$→
L_5; $[(a * b * c * d) . (a \supset b) . b] \equiv$
$1111000011110000 \equiv [(a \supset b) . b]$ (12)
M_5; $[A_1\ A_2\ A_3\ A_4]$ I, $[A_5\ A_6\ A_7\ A_8]$ IV,
$[a_1\ a_2\ a_3\ a_4]$ I, $[a_5\ a_6\ a_7\ a_8]$ II.

⑥ →
L_6; [Rests at L_5]. (13)
M_6; [Rests at M_5].

⑦ $_{c}$→
L_7; $[(a * b * c * d) . (a \supset b) . (b . c)] \equiv$
$1100000011000000 \equiv [(a \supset b) . (b . c)]$ (14)
M_7; $[A_1\ A_2]$ I, $[A_3\ A_4]$ III, $[A_5\ A_6\ A_7\ A_8]$ IV,
$[a_1\ a_2]$ I, $[a_3\ a_4]$ III, $[a_5\ a_6\ a_7\ a_8]$ II

⑧ ^{FS}→
L_8; $[(a * b * c * d) . (a \supset b)] . [(b . c) \vee (\sim b . c) \vee (\sim b . \sim c)] \equiv$
$1100000011001111 \equiv [(a \supset b) . (b \supset c)]$ (15)
M_8; $[A_1\ A_2]$ I, $[A_3\ A_4\ A_5\ A_6\ A_7\ A_8]$ IV,
$[a_1\ a_2]$ I, $[a_3\ a_4]$ IV, $[a_5\ a_6\ a_7\ a_8]$ I

Note that there is throughout an isomorphism between the binary selections in L and the mechanical sequence M if one takes those rods in the IInd, IIIrd and IVth positions as representing " o " 's, and those rods in position I as representing " l " 's. One can then read off the binary-table for the logical equation by reading along the sequence M from left to right.

The following is the operational schema corresponding to (8)-(15) above : ①$\xrightarrow{a}$ transfers the sequence $[a_1 \ldots a_8]$ containing the " excluded " " $\sim a$ " combinations to position II where they are stored ; this corresponds to formula (3), the eight " o " 's in L being analogous to the " $\sim$ p " alternatives in the last square bracket of this formula.

②$\xrightarrow{\supset}$ has no effect. In the case of a disjunctive antecedent, however, certain rods will have been stored in position III ; the operation of the copula key will return them to position I, so that during the working of the consequent they may be reconsidered.

③$\xrightarrow[b]{}$ transfers the sequence $[A_5 \ldots A_8]$ containing the rejected " $\sim b$ " combinations into position III. We have not yet, however, arrived at " $a \supset b$ ", but only at " $a \,.\, b$ ". The " excluded " " $\sim a$ " combinations which are still in position II only reappear at the next operation.

④$\xrightarrow{FS}$ returns the rods containing the " excluded " " $\sim a$ " combinations—the sequence $[a_1 \ldots a_8]$—to position I, where they become visible again, and finally transfers $[A_5 \ldots A_8]$ (*i.e.* the rods containing the " $\sim b$ " combinations inconsistent with the premise), to position IV.

⑤$\xrightarrow{b}$ transfers the sequence $[a_5 \ldots a_8]$ containing the " excluded " " $\sim b$ " combinations and stores them in position II.

⑥$\xrightarrow{\supset}$ has no effect (*cf.* ②$\xrightarrow{\supset}$ above.)

⑦$\xrightarrow[c]{}$ transfers the sequence $[A_3, A_4]$ $[a_3, a_4]$ containing the rejected " $\sim c$ " combinations to position III. We have not yet, however, arrived at " $(a \supset b) \,.\, (b \supset c)$ " but only at " $(a \supset b) \,.\, (b \,.\, c)$ " ; (*cf.* ③$\xrightarrow[b]{}$ above) ; the sequence $[a_5 \ldots a_8]$ containing the " excluded " " $\sim b$ " combinations still remains in position II.

⑧$\xrightarrow{FS}$ finally transfers the sequences $[A_3, A_4]$ $[a_3, a_4]$ to position IV, and returns the sequence $[a_5 \ldots a_8]$ containing the " excluded " " $\sim b$ " combinations (consistent with the premise) from position II to position I where they become visible. The working is now completed and the answer may be read off from the visible combinations on the face of the machine.

Throughout the " copula " (" $\supset$ ") key has had no effect, as it operates only on rods in position III. None of the antecedents considered has had the effect of putting rods into this position.

As we have seen, if the antecedent were complex (*e.g.* "$a \vee b$"), this operation would return to position I any combinations thus "stored" in position III. The account of the IIIrd position would then need to be expanded to cover such cases. It will also be noted that throughout the operations on a single proposition, the visible combinations are out of phase with the information actually impressed on the keys. Only when the "Full Stop" key is operated is there a strict correlation between the key sequence and the visible information. This is due to Jevons' device of storing "excluded" combinations.

When the premises have been fed into the machine it is possible to read off the combinations consistent with a given variable or its negation by pressing the antecedent corresponding to that variable. This operation has the effect of retaining in the Ist position only those rods which contain the variable in question, *e.g.* pressure of antecedent key "$\sim c$" will result in rods "a_7" and "a_8" being the only ones remaining visible. We are thereby informed that, given the above premises, "the absence of "c" is always accompanied by the absence of "a" and "b"," *i.e.* that "$\sim c \supset (\sim a \,.\, \sim b)$".

Figure IV summarises the visual state of the machine after the intervening operations have been effected by the keys pressed. The transfer-operations from one rod-position to another and the rods moved thereby are indicated below. "$\mathsf{T}\rightarrow$" signifies the transfer of rods; the numbers 1 to 8 the rod sequence $[A_1 \ldots A_8]$, and the numbers 9 to 16 the rod sequence $[a_1 \ldots a_8]$ (*cf.* (5) above). Then, *e.g.*:

$$\mathsf{T}\,\mathrm{II}_{12-16}\rightarrow\mathrm{I}$$

represents the transfer of the sequence $[a_4 \ldots a_8]$ from the IInd to the Ist position. The placing of the rod-numbers above the arrows indicates that the transference has no visual effect: "O" indicates that the key operation has no mechanical effect. The visual data is in the form of binary selections from the Logical Alphabet on the rods of the machine. The whole mechanical cycle of operations may thus be symbolised as in Fig. IV on opposite page.

One can easily reproduce the working of Jevons' machine by using his abacus technique (SS., p. 55-60 and App.). Write the combinations for the variables "a", "b", "c", and "d" vertically on the back of sixteen cards. This will give the sixteen combinations analogous to the letters on each of the rods. Arrange the cards horizontally side by side on the flat surface of a table. When working the problem place the selections

into the following positions (analogous to the rod positions), which should run vertically from the head of the table downwards, and be about 4 inches apart from each other: (1) Initial position of all the sixteen combinations (machine position I); when the

Key Operation	*Intervening operations*	*Final Visual State*	*Logical value*
① $\xrightarrow{\text{a}}$	$\top\text{I}_{(9-16)}\rightarrow\text{II}$	llllllllooooooooo	a
② $\xrightarrow{\supset}$	0	Repeats	a
③ $\xrightarrow[\text{b}]{}$	$\top\text{I}_{(5-8)}\rightarrow\text{III}$	llllooooooooooooo	$a\,.\,b$
④ $\xrightarrow{\text{FS}}$	$\top\text{III}^{(5-8)}\rightarrow\text{IV}\,\top\text{II}_{(9-16)}\rightarrow\text{I}$	lllloooollllllll	$a\supset b$
⑤ $\xrightarrow{\text{b}}$	$\top\text{I}_{(13-16)}\rightarrow\text{II}$	lllloooollllooooo	$(a\supset b)\,.\,b$
⑥ $\xrightarrow{\supset}$	0	Repeats	$(a\supset b)\,.\,b$
⑦ $\xrightarrow[\text{c}]{}$	$\top\text{I}_{\substack{(3-4)\\(11-12)}}\rightarrow\text{III}$	llooooooolloooooo	$(a\supset b)\,.\,(b\,.\,c)$
⑧ $\xrightarrow{\text{FS}}$	$\top\text{II}_{(13-16)}\rightarrow\text{I}\,\top\text{III}^{\substack{(3-4)\\(11-12)}}\rightarrow\text{IV}$	llooooooolloolllll	$(a\supset b)\,.\,(b\supset c$

FIG. IV

appropriate selections have been made the answer is read off from the remaining combinations; (2) corresponds to the "excluded" position II; (3) to store position III, and (4) to position IV where rejected combinations come to rest.

To obtain the solution of any problem compare the premises with all the combinations at (1). Select the combinations which are observed to be consistent with the premises and reject those found to be inconsistent, transferring these selections to the appropriate positions (as shown in, *e.g.* M_1 . . . M_8). Inference by this method becomes, as Jevons puts it, merely "The repetition of a few uniform operations of classification, selection and elimination of contradictories". For a large number of variables the card method is impracticable, and an electrical sorting device becomes necessary.

The Inverse Problem

The "Inverse Problem" follows from the method of "combinational logic" and the invention of logical machines. It arises irrespectively of the elements involved (terms or propositions), and may be expressed simply: given the consistent answers (*i.e.* combinations) resulting from the premises fed into

the machine—what is the most economical way of stating this end result? What is required is a unitary expression which has the highest deductive power or "logical force" (*cf.* SE., Ch. XXIV) (*i.e.* the most economical formula from which all the consistent combinations may be derived). For example, given the combinations:

$$(\sim a \,.\, \sim b) \vee (\sim a \,.\, b) \vee (a \,.\, b)$$

it will be seen at a glance that "$a \supset b$" is (to quote Venn, *Symbolic Logic*, Macmillan, 1881, p. 258) "a compendious summary of the solution of the problem". We thus start with the conclusion given by the machine, in the form of all such consistent combinations, *i.e.* the disjunctive normal form equivalent to the premises. The Inverse Problem seeks to find a way of expressing this selection in as economical a form as possible, methods of intermediate transformation or substitution being avoided as far as possible.

The main account of the problem in the *Principles of Science* (p. 134) appears independently of the account of the logical machine (p. 107); the connexion between the two is hence not very apparent, especially as Jevons takes the problem as a paradigm of the problem of induction. However, other publications[1] leave little doubt as to the connexion between the machine's solutions and this problem.

Though Venn thought it to be "the most original part of his logical investigations" (*ibid.* p. 256), those who took up the task of solving the problem after Jevons (*e.g.* J. N. Keynes *Studies and Exercises in Formal Logic*, 3rd edn., 1894, Macmillan, pp. 446-571 and Johnson, MIND, 1892, pp. 126-133), failed to see it in its true perspective in relation to logical machines. Jevons' alternative title for the question (namely that of "formal induction") and the contextual divorce of its statement from the account of the machine in the *Principles* makes this understandable. His view of "formal induction" was even more easily misunderstood because his conception of deduction as an example of the indirect method had not itself been fully appreciated. The Inverse Problem is used only to illustrate the problem of induction, and is rather to be understood as the inverse of deduction by the indirect method. Although the writers mentioned improved Jevons' method of solution, they were nevertheless unable to evaluate its precise significance for him.

[1] *E.g. Memoirs and Proceedings of the Literary and Philosophical Society of Manchester*, 2nd series, Vol. 5, and the Royal Society paper.

Such an evaluation required a whole logical perspective, evident to Jevons from his work on the indirect method and logical machines, yet foreign to his contemporaries who had not to grapple with the same problems.

With the re-appearance of logical machines, the problem is once again of interest, although it has been overlooked during the past fifty years. The constructors of a recent machine at Edinburgh (*Electronic Engineering*, April, 1951, pp. 126-133) seem to consider that each of the selected alternatives, taken singly, constitutes an " answer ", and apparently have no notion of the possibility of incorporating these alternatives into a single unitary formula. Eliminating technical details we are told that :

1. Once the problem has been reduced to a standard form, it may be solved by this machine. The solution will in general consist of finding all possible " answers ", that is to say, combinations of the variables which are consistent with all the rules (*i.e.* premises).

2. When a problem has been set up the machine is started searching for an " answer " by the pressure of the " think " [*sic.*] button, and it then scans the combinations.

3. On reaching such an " answer " the machine stops, the " answer " is noted and the machine is started scanning again by pressing the " think " button.

Thus this machine only gives its " answers " in the form of selections from successive columns of the binary-matrix, one by one, and apart from the technical ingenuity involved there is in principle little concrete advance on Jevons. Its constructors seem to ignore the fact, well known to Jevons, that the mere presentation of binary-cases (which is what the machine does) is the most complex manner of giving the solution. What is needed is an unitary answer in a more economical symbolism. The obtaining of such a conclusion constitutes the Inverse Problem.

An example will further clarify the point at issue. Suppose the premises of a given problem reduce themselves to a conjunction of the following propositions :

(i) *Premises :*	(ii) Binary matrix :	
1. $[(a \,.\, b) \,.\, \sim c] \vee (a \,.\, c)$	a; o l o l o l o l	
2. $(a \supset b) \vee c$	b; o o l l o o l l	(16)
3. $(\sim a \vee c) \,.\, (a \vee b \vee \sim c)$	c; o o o o l l l l	

A mechanical solution will reveal that those premises produce the binary-selection " l o l o o l o l " from the matrix (ii) shown above. (This is not the same matrix as (5) employed by Jevons,

the adoption of one particular matrix rather than another being a matter of local convention.) This selection is arrived at by taking the conjunction of the binary equivalents of premises 1, 2 and 3, using the rule for conjunction.[1] By employing Jevons' system of plotting the combinations which each premise excludes from the "Logical Alphabet" (*cf.* SE., pp. 214-226) we arrive at the same conclusion:

			Binary Value	
	$a.b.c$		1	
	$a.b.\sim c$		0	
	$a.\sim b.c$	3	1	
2	$a.\sim b.\sim c$		0	(17)
	$\sim a.b.c$ —1—		0	
	$\sim a.b.\sim c$		1	
	$\sim a.\sim b.c$		0	
	$\sim a.\sim b.\sim c$		1	

In scanning serially the consistent combinations (starting from the lowest combination in (17)), the Edinburgh machine would first show "$\sim a.\sim b.\sim c$" as an "answer", and then pass on to the next "true" combination, and so on in the same fashion, each combination being presented as a discrete and separate conclusion.[2]

However, each of the complexes here described as an "answer" is really only a single alternant of a group which as a whole is the logical equivalent of the unitary answer which the Inverse Problem's solution would provide, in this case: "$a \equiv c$". Alternants resulting from given premises were shown in full on the face of Jevons' machine, yet he did not lose sight of the possibility of a unitary equivalent. Like the Edinburgh machine, that of Kalin and Burkhart goes through all the complex alternatives serially, and notes the consistency or inconsistency of each alternative with the premise(s). Berkeley's description of the process (*Giant Brains*, J. Wiley & Sons, Inc., New York 1949, p. 161) runs: "The machine turns its 'attention' automatically to each line of the truth table one after the other, and pulses are fed in according to the pattern of assumed true statements. We can set the machine to stop on true cases or on false cases or on every case, so as to give us time to copy down whatever kind of result we are interested in. When we have noted the

[1] As will also the use of the rules given in "Note on the Exhibition of Logical Machines at the Joint Session, July 1950", *Mind*, April 1951.

[2] The problem of recording such "answers" is raised in the article quoted; the binary selection from a known matrix as in (16) is one solution to this; the solution to the Inverse Problem another.

case, we can press a button and the machine will then go ahead searching for more cases." This, despite its refinements, is no advance on Jevons. It is hence rather surprising to read : " In appraising the Kalin-Burkhart logical truth calculator we must remember that this is a first model. It was the only machine of its kind up to the end of 1948." This statement can be correct only if the reference is specifically to electrical relay machines. In general, the remarks made in connexion with the Edinburgh machine apply equally to the Kalin-Burkhart one.

Jevons' own method of solving the problem has certain analogies as he himself remarks, to the " sieve of Eratosthenes " employed by mathematicians to prove that a number is prime ; " there is no mode of ascertaining that a number is prime but by showing that it is not the product of any assignable factors. So there is no mode of ascertaining what laws are embodied in any series of combinations save by trying exhaustively the laws which would give them." (PS., p. 139.) Johnson (*ibid.*) however, worked out a simple method for translating the consistent combinations into a more economic formula by a system of " multiplying out ".

Today, against the wide background of digital computors employing the binary system of representation, Jevons' contributions fit in like the elements of a jig-saw puzzle. We find him waxing enthusiastic over the magical properties of the number two : " When contemplating the properties of this Alphabet, I am often inclined to think that Pythagoras perceived the deep logical importance of duality " (PS., p. 95). His views on the use of binary notational coding are also prophetic, and some of his remarks on universal systems of notation bear a strong resemblance to those of workers in the field of Information Theory (PS., pp. 192-196). It is fitting that the first electrical logical machine in this country was built at Manchester, and is in the University Department in which Jevons once exhibited his own machine.[1]

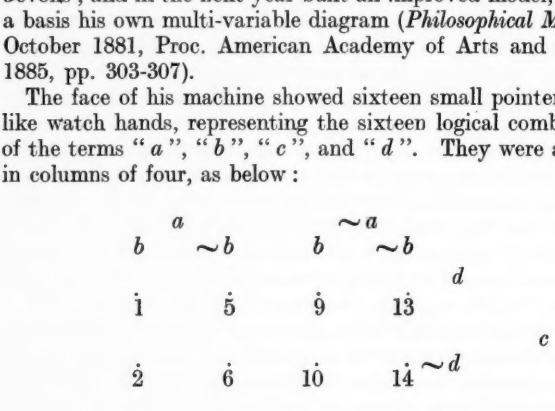
Note on the machine of A. Marquand

Jevons' machine, owing to the manner of its construction could only with difficulty be extended to cover problems involving more than four terms. At one time he contemplated building a ten-term machine similar to it, but found that he would have to sacrifice the entire space of one side of his library.

[1] Cf. W. Mays and D. G. Prinz, *Nature*, Vol. 165 (1950) p. 197, and W. Mays and D. P. Henry, " Logical Machines, New Light on W. Stanley Jevons ". *Manchester Guardian*, 14*th July*, 1951.

In 1881 Alan Marquand, of Princeton (formerly Fellow of Johns Hopkins), who had written to Jevons about his work, and whose name deserves to be remembered in the history of logical machines, constructed a device somewhat similar to that of Jevons', and in the next year built an improved model, using as a basis his own multi-variable diagram (*Philosophical Magazine*, October 1881, Proc. American Academy of Arts and Sciences 1885, pp. 303-307).

The face of his machine showed sixteen small pointers rather like watch hands, representing the sixteen logical combinations of the terms "a", "b", "c", and "d". They were arranged in columns of four, as below:



Each of the pointers indicated by the numbers 1 to 16 is analogous to one or other of Jevons' rods (*cf.* (10) above), but has the facility of indicating whether the combinations they represent are true or false. When at the horizontal position the pointer has the value "truth"; when at the vertical it has the value "falsity". Reading down each column in turn, the uppermost pointer at the left (1) represents the combination "$a.b.c.d$", the next one (2)—"$a.b.c.\sim d$", and so on until we reach the lowermost to the right (16) which is "$\sim a.\sim b.\sim c.\sim d$".

There are eight letter keys, and two operations keys indicated by "1" and "0". "1" raises all the pointers to the horizontal position and prepares the machine for a new problem, "0" causes the pointers to fall. When using the machine to solve logical problems, all the pointers are first raised to the horizontal position; this indicates the logical universe (or tautology) for four terms. The premise has, however, to be impressed upon the machine in a negative form. The premise "$a \supset b$", for example, produces exactly the same binary selection as the inserted

equivalent " $\sim(a \,.\, \sim b)$ ", or, as Marquand expresses it: " $a \,.\, \sim b \supset (o)$ ". The " a ", " $\sim b$ " and " 0 " keys are then pressed; on the machine's face the pointers representing the " $a \,.\, \sim b$ " combinations fall to the vertical position. When all the premises have been inserted, the conclusion can be read off from the vertical pointers as a contradiction expressed in the conjunctive normal form. If the remaining horizontal pointers are read instead, it is found to be expressed positively in the disjunctive normal form. Hence any conclusion given by the machine can be read either as a conjunction or as a disjunction.

The advantage of Marquand's machine over that of Jevons lay in its simplicity. For a ten-term machine Jevons would require 10,240 letters for his combinations and a keyboard with 44 keys. A machine of the Marquand type for ten terms needs only 124 letters and 22 keys. Baldwin tells us: " He also had designs made by which the same operations could be accomplished by means of electromagnets ".[1]

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[1] *Dictionary of Philosophy and Psychology*, Vol. II (The Macmillan Co., New York, 1902) Art. " Logical Machines " by the Editor (J. Mark Baldwin).

Since this article was written Marquand's manuscripts, now in the Princeton University Library, were examined by Professor Alonzo Church who drew our attention to certain material on logical machines in the collection. A photostat copy of Marquand's wiring diagram for a four-term electrical machine (presumably part of the design referred to by Baldwin) was sent to us.

Professor Church thought it important for us to see it, as it was not mentioned in any of the published papers he could recall. The diagram is of great interest; prepared about 1885, it is probably the first circuit of an electrical logical machine. It is wired in series and parallel, and among its components are sixteen electromagnets, eight one-way switches, one three-way switch, and a rheostat. Jevons' principles thus received their electrical translation at least half a century before present developments in relay computing machines.

(*Cf.* W. Mays and D. P. Henry, " Exhibition of the work of W. Stanley Jevons ", *Nature*, Vol. 170, 1952, p. 696.)

V.—DISCUSSIONS

THE IDENTITY OF INDISCERNIBLES AND THE SYMMETRICAL UNIVERSE

ARTHUR BURKS (*Proper Names*) and Max Black (*Identity*) have raised the possibility of the world being symmetrical, that is, the possibility of the world consisting of two sets of objects, each object being 'exactly like' an object in the other set. Such a possibility raises a serious problem and in this paper I propose to make yet a further attack on it even though some of my points have already been made by the authors mentioned. I shall show that the principle of the identity of indiscernibles is necessarily true in pragmatic languages, *i.e.* in languages containing egocentric words, and is at best contingently true in semantic languages, *i.e.* in languages without egocentric words. That is to say, the world might have been of such a character that two objects in it would differ in some respect which could not be mentioned in a semantic language. It will follow that although a semantic language is adequate for the formulation of a complete state description no matter what the world is like, only a pragmatic language is capable of recording a particular matter of fact. In the case of pragmatic languages there is what might be called a *token-link* between language and the world. This paper concludes with an introductory examination of the token-link.

I

The principle of the identity of indiscernibles may be taken to mean that if two objects O_1 and O_2 are numerically different then they are qualitatively different, they differ in some mentionable *respect*. But we can construct a universe for which this principle is in some sense false. Consider a universe consisting of two identical sub-universes related symmetrically about the midpoint between them. In some sense or other every object in one sub-universe has a numerically distinct correlate with which it shares all its qualities and relations. Let us consider two such corresponding objects O_1 and O_2 and two corresponding observers Smith_1 and Smith_2. In some sense O_1 and O_2 are numerically distinct but qualitatively identical. There can be no quarrel over this. The task at hand is to elaborate a sense of 'qualitatively identical' such as will obviate any quarrel over the foregoing statement. In other words, we shall want to say that O_1 and O_2 have all their properties in common and we must place enough restrictions on the range of the predicate variable to permit us to say it.

We can begin by excluding such alleged properties as that of *being identical with* O_2 and that of *being different from* O_2. The

reason is obvious: you might claim that O_1 and O_2 are numerically different and be asked to support your claim by mentioning an unshared property. If you cite the property of being numerically different from O_2, which is possessed by O_1 but not by O_2, then your answer is clearly a *petitio*. Now $Smith_1$ will be able to differentiate between O_1 and O_2 since O_1 and O_2 each bear different relations, R and S respectively, to him. By hypothesis, O_2 bears R to $Smith_2$. It might be said that O_1 and O_2 differ qualitatively in that O_1 but not O_2 has the property of bearing R to $Smith_1$. Clearly the earlier restriction will be ineffective unless we also deny that there are such properties as that of bearing R to $Smith_1$. We can combine both these restrictions in the following: there are no properties whose mention requires the use of an individual constant. We shall of course admit such properties as that of bearing R to something which is P.

A more complicated and perhaps more revealing way of getting around the difficulty is this: We admit the properties, *bearing R to* $Smith_1$ and *bearing R to* $Smith_2$, and hold that they are numerically different but qualitatively identical properties. Those of us who wish to maintain that O_1 and O_2 are qualitatively identical will construe 'qualitative difference' to mean not simply the possession of numerically distinct properties but the possession of qualitatively distinct properties. The claim that *bearing R to* $Smith_1$ and *bearing R to* $Smith_2$ are qualitatively different would have to be supported by mentioning some qualitative difference between $Smith_1$ and $Smith_2$. On our hypothesis this attempt would lead to a vicious regress. The reason for this further restriction is this: Suppose God were to instruct Gabriel to enter the world and stand as guardian angel over O_1. Gabriel might ask, 'Which object is O_1?' If he is answered by the statement that it is the object which bears R to $Smith_1$ the answer will be uninformative unless there is an informative answer to the question, 'Which individual is $Smith_1$?' And there is not an informative answer to the latter question. Hence either of the suggested alternatives (further restrictions on the range of the predicate variable in 'All properties' *or* a more stringent interpretation of 'qualitative difference') would be reasonable. They are reasonable in view of the fact that we are engaged in explicating what would be meant in saying that O_1 and O_2 though numerically different would 'look exactly alike' from a vantage point not localized within the universe.

It is perhaps worth noting parenthetically that it might be possible to raise much the same issue in terms of an eternally recurrent universe, one whose history is divided into long cycles all mutually identical. Russell (in the *Inquiry*, A427, B337 and in *Human Knowledge*, p. 295) recognizes the possibility of such a universe and explicitly assumes that the world is not eternally recurrent, that is, he assumes that some experiences are qualitatively unique. Since our everyday judgments are not made on this

assumption, there is a question as to whether Russell's analysis is of any interest even if the assumption happens to be true. I have chosen in this paper to consider a symmetrical universe because there is some doubt as to whether 'eternal recurrence' is meaningful. It might be held that if cosmic history is cyclic then it is merely that the universe is finite but unbounded in its temporal dimension. The cycles might be taken as *numerically* identical. We should then have a certain event E so many billion years before itself in much the same way that Chicago is so many thousand miles west of itself. In other words, eternal recurrence would not compel us to admit the possibility of bare numerical diversity. But I do not think there is any doubt about the meaningfulness of supposing that the universe might be symmetrical. I am supposing that the two identical sub-universes are so related that a person might, in principle, pass from one to the other, that he could confirm the hypothesis that the sub-universes are qualitatively identical, that he could significantly entertain the hypothesis that they are not exactly identical. We can even imagine Smith_1 and Smith_2 shaking hands, provided their hands clasp about the point of symmetry.

Having established the meaningfulness of the supposition that the world might have been symmetrical we can proceed to draw some conclusions. I have already mentioned that Smith_1 can observe (discern) the difference between O_1 and O_2. He *can state* the difference in terms of their different relations to him*self* or in terms of their different relations to something he calls 'this'—that is, in sentences containing mention of egocentric particulars. I conclude that the principle of the identity of indiscernibles is necessarily true for pragmatics and contingently true for semantics. More precisely, it is *necessarily true* that numerically distinct individuals differ in a respect that can be mentioned using egocentric words but only *contingently false* that there are numerically distinct individuals which do not differ in a respect that can be mentioned without using egocentric words. I have, in effect, simply defined 'qualitative difference' in such a way as to make the foregoing statement true.

The foregoing considerations lead Burks (in *Proper Names*, p. 42) to conclude that proper names are really abbreviations for what he calls indexical definite descriptions, which have the form, 'This P_1'. I cannot subscribe to all the details of his account of proper names but he is clearly right in inferring that proper names in used languages have some egocentric reference. They cannot be synonymous with descriptions of the form, 'The x which is P_1', for if the world were symmetrical all such descriptions would be empty and in using proper names of individuals we do not claim that the world is not symmetrical even though we may believe that it is not. In speaking of a name having egocentric reference I mean that the circumstances of the use of a token contribute to its meaning. I

shall speak of the *token-link* between a language and the world. In discussing the symmetrical universe I used the names 'O_1', 'O_2', '$Smith_1$' and '$Smith_2$' without supposing they had any egocentric reference. It was because of this omission that the account of qualitative difference was perhaps unnecessarily complicated. Had it been supposed that a proper name must have egocentric reference and that a strictly semantic language must therefore contain no individual constants, then some of the difficulties would never have cropped up in the first place.

By a strictly semantic language, I mean one completely abstracted from the circumstances of its use or mention. In constructing a language the pure semanticist generally specifies the designata of individual constants by means of metalinguistic individual constants and the egocentric reference of the latter precipitates itself into the object language. With this procedure we do not have a strictly semantic language. Carnap's co-ordinate languages would appear to be an exception but only because they are not fully interpreted. He speaks of his individual expressions —quadruples of real number expressions—being correlated with positions (*M & N*, p. 86) but the language is not interpreted unless this correlation is supplied. To supply the correlation we might specify the origin as the position of the individual which is P—in which case we are making a factual claim that precisely one individual is P—or we might specify the origin as the position of the individual *a*—in which case we are using a metalinguistic name, whose egocentric reference will precipitate into the object language.

It is clear, then, that a strictly semantical language cannot contain individual constants although it may contain individual variables. It will permit the formulation of descriptions and provided the world is not symmetrical these will occur in some true sentences. But such sentences are not both decisively verifiable and decisively falsifiable. In ordinary language singular sentences containing proper names are presumably both—neglecting the deeper epistemological problems having to do with incorrigibility. In other words, a strictly semantic language cannot record particular matters of fact. We can summarize our results so far by saying that a language cannot be abstracted from the circumstances of its use or mention (which is what we aim to do in semantics) without working considerable violence on it.

II

In the remainder of this paper I shall investigate the nature of the token-link between language and the world. There is another, a more specific problem which will claim our attention, that of uniqueness. Returning to the symmetrical universe, we can say that O_1 is not qualitatively unique since it has a numerically distinct duplicate. But it is numerically unique: it is just that individual and no other. There will be the problem as to how—to

put it roughly—a judgment manages to catch hold of the numerical uniqueness of the object judged, how, in other words, it can refer to just that object and not its duplicate, assuming it has a duplicate.

To begin with a definition: if a certain thing is the object of attention (whatever that means) and occasions a judgment expressed by a sentence of the form, 'This is ——', then (following Wilfred Sellars, *RNWW*, p. 434) the token of 'this' *confronts* the object and the object *stands opposite* the token. Does an object as denoted by 'this' have an identifying (*i.e.* essential) property? If it does not, then the token of 'this' is a genuine proper name; if it does then the token will be an abbreviated definite description of some sort or other. In what follows I shall assume that the object designated has an identifying property, namely, the property of standing opposite the token of 'this' which denotes it. Thus we can imagine a person (whom I shall call 'Charles') recording an immediately apprehended fact in a sentence like the following:

(1) The x which stands opposite *this* is red.

Here the token of 'this' is self-referential. It means itself in a mode of meaning that is *sui generis*. Alternatively we may imagine Charles saying:

(2) This is red

where 'Thi' is construed as an abbreviation for 'the x which stands opposite' and the terminal 's' is taken as a self-referential token.

Now suppose I observe the pencil on my desk and judge it to be red. I am making a judgment about that (numerically) unique object. It may be—as was shown by the argument from the symmetrical universe—that in the remote depths of space there is another object with all the properties of this pencil, that another person with all my properties judges *it* to be red. There must be something in my statement to particularize the reference to *this* object. This is accomplished by the self-referential token of 'this'. A token of 'the x which stands opposite *this* is red' succeeds in recording a particular fact because the token of 'this' is that unique token. (It may have a qualitative duplicate.) In other words, a token of 'this' conveys the uniqueness of objects not by *meaning* numerical uniqueness, not, that is, by specifying the respect in which anything is unique, but by *exhibiting* its own uniqueness. It is that token and no other. I am inclined also to believe that if there is a sense of 'existence' (or, 'actuality') not caught by Quine's well-known criterion, then it is a sense which is unformulable but can only be conveyed by a token which *exhibits* its own existence.

The foregoing can be considered only as an introduction to the problem of egocentricity, not as a final account. In the first place the assumption that an object denoted by a token of 'this' has as its identifying property the property of standing opposite the token was a more or less arbitrary assumption which should be dispensed

with. Secondly (and more seriously) if the meaning of every sentence token is anchored to that token, then no two sentence tokens will mean the same thing. Communication becomes a mystery. The problem of fugitive propositions is a special case of the general problem. The puzzle, be it noted, is not as to how we communicate, but as to the construction of a definition of 'communication' which will permit us to say that some people communicate. [In a subsequent paper on egocentricity and communication I shall endeavour to diffuse the token-link between language and the world in such a way as to permit us to say that some distinct sentence tokens have the same meaning.] My purpose in this paper has been to show that there must be a token-link between the world and a language capable of recording singular facts and to illustrate the kind of link which may subsist. The earlier result concerning the identity of indiscernibles may now be reformulated: Discernibly different objects are numerically different; but if the world were symmetrical then two objects might differ in a way that could not be stated in a language which lacked a token-link with the world.

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VALUE-JUDGMENTS

MR. TOULMIN and Mr. Baier devote their recent paper "On Describing"[1] mainly to questions of English usage; they explain the ordinary use of the word "description" and show how some philosophers would diverge from it. The philosophical question involved here, is not entirely ignored. It is, of course, the question, whether that divergence of philosophical from ordinary usage may serve a useful purpose. Mr. Toulmin and Mr. Baier think that it may not. Some philosophers take it to be important to distinguish two types of sentence—"descriptive" and "non-descriptive"; Mr. Toulmin and Mr. Baier not only disapprove of the terms in which the distinction is *expressed*; they consider the distinction itself to be "illegitimate" (p. 33).

I wish to try to show that, in the sense in which it is usually made, the distinction has a legitimate and important function, although it would, no doubt, often enough also serve less "legitimate" purposes.

I

The main purpose of the distinction, as usually recognized, is to distinguish the sentences of which we can, from those of which we cannot use the words "true" and "false" (pp. 35 f.). It may be misleading to speak of the former as "descriptive" and of the latter as "non-descriptive", but the philosophically significant question is whether or not there are good reasons for making such a distinction, and more especially, whether there are any important types of statement which would have to be characterised as "not true-or-false". There are many claimants for inclusion in this category. Mr. Toulmin and Mr. Baier concentrate on one of these: statements expressing value-judgments; and they consider the claim here to be illegitimate. Value-judgments, they think, *are* true-or-false: ". . . if in a murder-trial the foreman delivers the verdict, 'We find the prisoner guilty', the prisoner may protest, 'It's not true! I didn't do it!', with perfect logical propriety." For, ". . . it is in the nature of the law that verdicts should often imply things about what happened at the material time: so considered they can very naturally be criticised as false . . ." (p. 36). There is no reason, they argue, for any utterance to be "disqualified from being true or false". And further, although "it is true, as Mr. Hart says,[2] that the judge is not required 'to make deductive or inductive inferences from the

[1] MIND, January 1952, pp. 13-38.

[2] H. L. A. Hart, *The Ascription of Responsibility and Rights* (Proceedings Aristotelian Society, 1948-49, pp. 171-194, reprinted in Flew, *Essays on Logic and Language*).

statement of facts' . . . he is none-the-less required to make the *appropriate* kind of inferences: and, as with all inferences, we can ask two kinds of question about the conclusion, 'Is it true?' and 'Does it follow?' . . ." (p. 37).

Thus, two reasons seem to be here being offered for placing value-judgments in the category of true-or-false statements: such judgments are said (1) to entail, and (2) to be entailed by, true-or-false statements. No doubt these are good reasons. But so far as I can see, no argument is offered for accepting them, and none for controverting the many (*e.g.* Mr. Hart's arguments) that have been advanced on the other side.

II

I do not wish to deny that value-judgments entail *some* statements whose true-or-false character (or, as we might say, 'propositional status') is undisputed. For example, *X is guilty of having murdered Y* does certainly entail that Y is dead. But to prove their case, Mr. Toulmin and Mr. Baier ought to show that their other reason for regarding value-judgments as true-or-false is also valid: that these judgments besides *entailing* true-or-false statements are also *entailed by* them. In other words, they must show that there are some propositions whose truth is not only a necessary, but also a sufficient, condition for the appropriateness of a value-judgment. Unless they can do this, they will only have proved that value-judgments may be considered to be false, but not that they may be considered to be true, or true-or-false. This cannot possibly be shown to be the case. Amongst the propositions which are necessarily true, if a value-judgment is appropriate or right, there are none which are necessarily false, if it is wrong. A judgment like *X is guilty* or *Y is beautiful* can always be invalidated (or, as Mr. Hart would say, 'defeated') without either invalidating the procedure by which it was obtained or refuting any of the propositions which it entails. To deny a value-judgment does not necessarily mean that any conditions of its appropriateness (or 'adequacy') are absent; it may mean merely that these have been *added* to. In a murder-trial, for instance, the judgment *X is guilty* may be 'defeated' by additional conditions such as: that X was a surgeon, or a soldier, or the driver of a train, or insane, or that he acted on provocation, or in self-defence, or that there was an accident—an immensely wide range of facts which, being incapable of general definition, can only be indicated; the indication itself being often of the same status as *X is guilty* (as, *e.g.* when we say "a surgeon in the careful and skilful performance of an operation" or the like.)[1]

[1] A stimulating discussion of the problems here involved is found in J. T. Wisdom's *Gods* (Proceedings Aristotelian Society, 1944-45, reprinted in Flew, *Essays on Logic and Language*).

If we chose to say that a value-judgment was 'true', whenever we considered it to be right, and that it was 'false', whenever we considered it to be wrong, we should be faced with the queer case of statements which may be 'false' in the presence of all the conditions of their 'truth'.

It is impossible to remedy this non-logical (but by no means illogical) behaviour of value-judgments by trying to include the *negation* of invalidating conditions amongst the necessary conditions of a right or appropriate judgment. This is impossible, not only on account of the undefinably wide range of those invalidating conditions, but also, and more significantly, on account of the intrinsic nature of negative conditions. For to say that a negative condition (the absence of a 'defeating' fact) is fulfilled, could only mean that the field of inquiry had been extended far enough for that denied fact to appear, if it were relevant to the case in hand. But how are we to know that the field of inquiry has been extended far enough? There are no rules of logic or scientific procedure to tell us. The decision is not a matter of logical or experimental analysis. It is ultimately a verdict of the imagination.

Thus, when a value-judgment such as *X is guilty* is claimed as right, the claim is not merely that it entails certain necessary conditions (*i.e.* propositions asserting the presence of such conditions), but also that the field of inquiry had been extended far enough for any 'defeating' conditions to appear, and that none did appear. The judgment can always be reversed without denying any of the conditions which it entails—merely by extending the range of inquiry and discovering *additional* conditions.

This is why we can never have logically *sufficient* reasons for a value-judgment, and why it is dangerously misleading to call any such judgment a 'conclusion', and definitely false to present it as a logical conclusion. We are free, of course, to describe it as *some sort* of conclusion arrived at by *some sort* of inference (neither deductive nor inductive, but just "appropriate"). Ordinary English permits such expressions. But if we assumed that in using *them*, we were speaking the language of Logic (*some sort* of logic: "the logic of value-judgments"), then we should merely be creating a philosophical muddle, though a muddle expressed in correct English.

Similarly, we may, without offending against English usage, speak of the 'truth' or 'falsity' of a value-judgment, when we wish to refer to the reasons we have for asserting or denying, accepting or rejecting it; but it would be the merest confusion to assume that this is sufficient to prove that a value-judgment is true-or-false in the same sense in which, for instance, a judgment of perception is. Appeal to ordinary usage does not remove the important difference between (1) a proposition, for the assertion of which there are sufficient conditions, and the denial of which entails the absence of some condition of its truth, and (2) a value-judgment, for the assertion of which there are no sufficient conditions, and the

denial of which is possible in the presence of all the conditions of a valid assertion of it.

Mr. Strawson once made the irrefutable statement that "it is correct to say that utterances of any kind are true or false, if it is correct usage to signify agreement or disagreement by means of the expressions 'true' or 'false'";[1] and, of course, it *is* correct . . . if it is correct usage. But is it good philosophy? Is it advisable to slur over the difference between (1) statements which admit of logical argument for obtaining agreement or disagreement, and (2) others which are not true or false in this sense? Shall we perhaps (as is suggested by those who speak of "the logic of . . ." in a variety of senses) adopt a notion of 'logical argument' that would be loose enough to admit of *all* 'correct uses' of 'true' and 'false'? There is nothing to prevent us from defining 'true' and 'false' exclusively by reference to 'ordinary usage'; but then we shall have Rhetoric instead of Logic, and any 'appropriate' rhetorical device for gaining agreement or disagreement will masquerade as a logical argument.

Nostalgia for the unproblematically correct language of John Doe and Richard Roe is no help in philosophical inquiries. When they tell us that to them 'true' simply signifies agreement, and 'false' disagreement, we are not helped in the least; we are still left with all our problems unsolved—amongst them the problem of value-judgments, where the disjunction of agreement and disagreement, and of valid agreement and valid disagreement, "does not cut along the same line" as the properly logical disjunction of true and false.

A value-judgment entails some propositions; the falsity of any of these entails that the judgment is wrong. But no conjunction of propositions entails a value-judgment; there is no proposition whatever, the falsity of which is entailed by the judgment being wrong. Of all the conditions, the presence of which is necessary for the judgment to be right, none need be absent for the judgment to be wrong. In other words, valid denial does not exclude any conditions of valid assertion. (It might be advisable, in referring to such judgments and their negations, to revive an old term and speak of 'opposites' rather than speak simply of 'contradictories' or 'contraries'.)

The terms 'true' and 'false', in the sense in which they apply to judgments of perception or any other judgment of analysis, are far less significant here. We may have occasion sometimes to say of a value-judgment that it is *false*. We shall, if a necessary condition of its being right, which was thought to be present within a certain range of fact, is found to be absent from it. But we shall never be able to say of any value-judgment that it is *true*. A value-judgment, though it may be false, is never true-or-false. When we assert or deny, accept or reject it, we are *not* deciding whether it is true or false.

[1] P. F. Strawson, *Truth* (Analysis, June 1949, p. 94).

To say this, however, is not the same as to say that in making such a decision we are not referring to objective facts, or that 'right' and 'wrong' here signify nothing more than different ways in which we may *feel about* facts. The decision between 'right' and 'wrong', as referring to a judgment, depends on what we see, not on how we feel about what we see. But it is a decision also as to what to look at.

III

The verdict that X is guilty, or that he is not, depends not only upon an examination or analysis of either X (as when we say that X has blue eyes) or his action (as when we say that he has killed a man); it depends also and essentially on an imaginative decision as to what *else* to look at. There are, it is true, rules of procedure which aim at securing as far as possible, the 'propriety' of such a decision; but if the decision is over-ruled and the judgment reversed, this does not necessarily imply that some 'performatory rule' of procedure had been infringed, or that anything entailed by the previous judgment is now denied: it may mean merely that X's guilt rather than innocence, or innocence rather than guilt, did appear within a context which was wider than the one in which X and his action had been placed before. The same facts, we may say in that case, have been re-interpreted in a new context.

Sometimes it may happen that new conditions *accrue*. For instance, a man dies of an injury he had sustained some time earlier. We may say then that the facts *require* to be re-interpreted. Such is the case also, when something that was beautiful fifty years ago is ugly today; or when a scientific hypothesis (which for very similar reasons is not true-or-false) ceases to be adequate.[1]

It is this essential decision on the context, delimiting the field of inquiry, which excludes value-judgments from the category of true-or-false statements. Only judgments expressing an analysis within fixed boundaries are true-or-false. Yet judgments whose appropriateness depends on a decision as to the *scope* of the analysis as much as on its *results*, are right or wrong, good or bad, sound or unsound, appropriate or inappropriate, adequate or inadequate; they are not, in any logically legitimate sense, true or false.

This applies to every kind of value-judgment. The beauty of a flower is nothing to be picked like the flower, or to be picked out like its colour or shape. Aesthetic inquiries of the illuminating kind do not ask whether some feature or other *has* beauty or not; their whole endeavour is towards a decision as to the context in which a thing *displays* beauty, or, on the other hand, in which its

[1] An explanatory hypothesis and a value-judgment have very similar logical relations to true-or-false factual statements. See S. Körner, *On Some Moral and Other Concepts* (Philosophy and Phenomenological Research, 1950).

beauty is *defeated*. Always it is the addition or subtraction of *other* things that makes all the difference between beauty and ugliness of a thing. In order to see beauty or ugliness, as truly as in order to see innocence or guilt, we must do more than pick out a quality from a thing; we must *place* the thing—see it in as wide and rich a context as our powers of imaginative appreciation demand. This is why it is so much more difficult to judge aright than it is to tell the truth.

There are, of course, important differences between various types of value-judgment—legal, moral or aesthetic. The procedures available for determining the proper scope of sound judgment may be more or less reliable, and the subsequent analysis of the relevant field may be more or less precise. The *cognitive value* of our judgment depends on the reliability of those procedures and the precision of that analysis; but its *appropriateness* is never wholly determined by either. The soundness, the justness, the adequacy, the appropriateness of our moral or aesthetic judgments will always depend on how nearly complete a view we have gained of the relevant situation, and the delimitation of our field of inquiry must always be largely a matter of imaginative choice.

The facts referred to in moral and aesthetic judgments are as 'objective' as any. Kindness, wilfulness, beauty, or ugliness, although inaccessible to any purely analytical procedure, are on that account no less objective than heat, cold, light, or darkness. The imaginative effort, on which the appearance of values depends (as the appearance of colours depends on the opening of our eyes) is wholly engaged on objective fact; it merely determines the context of factual relevance. And our judgment *describes* the values which appear in that context. To know whether the description is right or wrong, we look at facts, and facts only. It is wrong, if analysis of the 'given' field proves some factual references to be false, or if expansion of the field reveals further relevant facts which don't fit the description.

It is misleading, then, to speak of a distinction between 'descriptive' and 'non-descriptive', when what we have in mind is the distinction between sentences which are true-or-false, and others which are not. Some philosophers, it is true, do hold that judgments which are not true-or-false, and more especially value-judgments, describe nothing. This view, it seems to me, is patently false. And in repudiating it, Mr. Toulmin and Mr. Baier would have been on perfectly safe ground, had they not coupled their protest against the unjustifiable implications of the distinction between 'descriptive' and 'non-descriptive' sentences with a much less defensible protest against its most important and legitimate function—namely, to distinguish *propositions*, which are true or false, from other judgments which are not.

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PAYING HEED

In a recent article in MIND, Mr. Campbell Garnett criticizes Professor Ryle for his discussion of 'heed-concepts' in *The Concept of Mind*. Mr. Garnett's contention is that words like 'heeding', 'minding', 'trying', and 'attending', have now to be 'recognized as referring to distinct observed episodes or events within the complex whole of experience'. In opposition to Professor Ryle's analysis of what might be called heed statements as 'mongrel-categoricals', Mr. Garnett argues that heeding, or attending, must be episodic because anyone who is asked whether or not he was attending to what he was reading, can answer not only by seeing whether he can remember what he has read, but alternatively by directly recalling the experience of reading and remembering having been at the time deeply interested and attentive.

Although Mr. Garnett is right in adducing these facts, I would suggest that he is wrong in the conclusions which he draws from them. For these facts can equally well be explained by an analysis of 'heeding' and 'attending', as essentially negative terms. That is, to say that I was attending to what I was doing, whether it be reading or playing tennis, is just to say that I was not thinking about anything else at the time. The more I thought about other things, the less attentively was I reading or playing tennis.

Now if this analysis of heeding as a negative term is right, it would follow that in some respects doing something attentively is less sophisticated than doing it inattentively—although not, of course, that doing something efficiently is necessarily less sophisticated than doing it inefficiently. And that this is so can be seen by considering children, who, when young, might be said to do everything attentively. To talk of a child playing with toy soldiers inattentively seems inappropriate; and to ask him to go on playing but at the same time to 'keep half an eye' on something else, is to ask for the impossible. It is only at a relatively late stage that a child is able to conduct one operation with his hands and carry on an entirely unrelated internal soliloquy at the same time. We have to learn to do things attentively before we can do them carelessly, heedlessly, and inattentively.

Now it is true, as Professor Ryle points out, that often we decide whether or not someone has been doing something attentively by observing the results; if the operation has been conducted less efficiently than one would have expected, one is inclined to say that the person concerned was not paying heed to the job in hand. And this inference is justified because usually, if people attend to what they are doing, they will on different occasions reach roughly the same level of efficiency. But nevertheless it is an inference

and like most inductive inferences it sometimes breaks down. We have all occasionally found ourselves concentrating on some task but yet, for some reason, failing to do it nearly so effectively as usual.

A corollary to the view that I am suggesting, is that what are normally thought of as degrees of attention would more properly be called degrees of inattention. Sometimes we read without ever pausing to notice the fact that it is getting near lunch time or that there is a pneumatic drill working outside, sometimes our thoughts dart off every few minutes to some quite unrelated subject. To be perfectly attentive is just to notice or think of nothing that is not relevant to the job in hand.

It might seem that attention does not vary only in temporal extent or continuousness, but also, at any one instant, in intensity. But this, I think, is wrong. It is true that on certain occasions such as watching the exciting finish to a boat race, or some gripping tragedy at the theatre, we may think that we are attending more intensely than we ever are when playing bridge; but this is only because in such cases an extra emotional element enters. As far as attending is concerned, we are attending just as much if we spend ten minutes thinking about nothing except the right bid, the right lead, whether to finesse, and how to squeeze our opponents, as if we spend the same length of time excitedly watching Oxford row the last mile to beat Cambridge by quarter of a length.

That attention is seldom completely undivided is recognized by Mr. Garnett himself and is a fact adduced in the course of his argument. For he holds that paying heed to whatever we are doing is a separate mental episode and that we know that we are paying heed by noticing it at the time. 'There is no infinite regress involved in the concept of noticing our own noticing. Only at moments of extreme concentration, if ever, is our attention completely undivided.' And this would presumably lead to the conclusion that although normally we can notice at the time our activity of paying heed, when it is at its most intense (at 'moments of extreme concentration') we are unable to do so. It seems more plausible to maintain that paying heed is not a mental episode at all.

What I have said here applies only to words like 'heeding', 'attending', and 'minding', not to terms like 'striving', 'purposing', and 'trying', which both Professor Ryle and Mr. Garnett seem to think operate in exactly the same way, Mr. Garnett speaking of the difference between 'a heedful or intentional and an unheedful or unintentional act'. But, of course, some acts, like lighting and smoking a cigarette, may be intentional but unheedful; and others heedful but unintentional, as when we suddenly find that we are listening attentively to someone else's conversation. The analysis of terms of purpose must therefore proceed on quite different lines from that of words like 'attentively' or 'heedfully'.

Mr. Garnett's conclusion that all cognition is a form of trying and that trying is episodic, may or may not be tenable; he is, however, wrong in seeking to establish these conclusions by arguing that attending is a special mental activity and assuming that trying is the same as attending.

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NOTE ON THE ENGLISH VERSION OF WITTGENSTEIN'S *PHILOSOPHICHE UNTERSUCHUNGEN*

IF this had been any other book I should have held on to it for a year or so after finishing the translation. As it was, it went to press piecemeal as it got revised. I have to thank the publishers and printers for their patience in incorporating corrections up to the time when the presses were locked up. I saw certain further mistakes after this date. The following are corrections of faults in the translation which must, or might, prevent a reader from understanding.

p. 47e, § 109 for *power of illumination* read *light.* Wittgenstein is referring to the fact that to people not concerned with philosophical problems his descriptions often seem obvious enough, but boring and pointless; whereas to someone so concerned they can seem full of light.

p. 52e, line 7 for *is employed only* read *gets employed.* § 134 is spoiled by the fact that there is a distinction in English usage between "sentence" and "proposition"; German has only one word, "Satz".—In rendering "Es verhält sich so und so": "This is how things are", I naturally do not intend any criticism of the *Tractatus* rendering: "Such-and-such is the case" (which I kept here for "Das und das ist der Fall"). The correction I give here is mere compensation for the fact that "This is how things are" is *not* "only used as a propositional variable"; it is merely sometimes so used. An advantage of the rendering "This is how things are" is that it does not sound absurd to call this a "proposition" (required at certain parts of the paragraph) whereas it does sound absurd to call "Such-and-such is the case" a "proposition". (Wittgenstein here suppressed as superfluous a remark that *e.g.* "The sky is blue" could have been used as a propositional variable.)

p. 76e, § 189 for *aren't the steps* read *are the steps then not.*

p. 110e, line 5 for *reactions* read *reaction.*

p. 120e, line 6 delete *in order to imagine it. And.*

for *for* read *For.*

line 7 for *point* read *point either.*

p. 137e, § 493 for *Doesn't it look quite different* read *Isn't the aspect quite altered.*

What is in question here is the connexion with the topics of II xi.

p. 143e, § 527 for *linguistic phrase* read *sentence.*

p. 153e, end of § 582 for *feelings* read *feeling.*

p. 165e, § 644 for *what I did* include *the intention* read *the intention lie* also *in what I did.*

p. 172e, § 692, line 4 for *does* read *did.*

p. 207e for *aspect* read *aspects.*

p. 211e, last line for '*talking to oneself*'—*I do not mean out loud* read '*inward speech*'—*I do not say "to oneself"*.

The point of this is made clear by a remark occurring in the manuscript from which Part II is a selection: "I ought not to say 'silent talk to oneself', for it is possible to talk inwardly without talking to oneself."

p. 215e, line 5 for *a word* read *the word.*

p. 216e, line 10 for *That* read *It.*

I think this must be right. I am perplexed by it, however, because Wittgenstein certainly did not think that the existence of 'word-experiences' "altered nothing"; and it is not perfectly clear that one could identify it apart from the language-game in which we use the word "meaning" as here described.

p. 217e, line 11 for *to oneself* read *silently.*

The same point as on p. 211. There are a few other places in this fragment where I put "to oneself" instead of "silently" because I had no idea of the distinction.

I believe that the corrections listed above have been incorporated in the American edition. The following have not:

p. 46e, § 108, line 1 for *have* read *has.*

line 2 for *are families* read *is this family.*

p. 90e, § 251, last line for *a proposition a priori* read *an a priori proposition.*

p. 98e, § 284, line 9 for *consist in* read *arise from.*

line 10 for *the living behave in such and such a way and the dead do not* read *a living thing moves in such and such ways and a dead one not.*

p. 155e, § 591 throughout for *veering* read *tending.*

I have inserted a footnote to the end of § 32 in the American edition: "Wittgenstein later rejected this; see *e.g.* p. 217. He asked me to make such a note. G. E. M. A."

This needs a little explanation. As 'thinking' and 'inward speech' are concepts quite different in kind, it is doubtful whether Wittgenstein has the right to make the comment at the end of § 32. He was concerned, in the last few months of his life, to think that he had, somewhere in this book, written as if there were not this difference, and he asked me to make a note on it if this were the case: he had, he said, formerly not realised the difference. I believe that § 32 deserves this note: but I ought to say that Mr. Rhees (whom I had not time to consult before sending it in) does not agree with me.

Erklärung means both "explanation" and "definition". Readers may like to reconsider the rendering in some places.

Übersehen, "to see over", with its cognate *übersichtlich*, "lucid" seems impossible to render in English; I have rendered it by "command a clear view", "survey"; and at § 122 have rendered *übersichtliche Darstellung* "perspicuous representation". *Übersehen* suggests that something can be taken in at a glance because it is well arranged. It may be helpful to note that in speaking of a "perspicuous representation" Wittgenstein is referring, not to the general course of the book but to the individual sections or groups of them taken together. (See § 5.)

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VI.—CRITICAL NOTICES

God and Nature. By G. F. STOUT. Cambridge University Press, 1952. Pp. liv + 339. 35s.

THIS book is prefaced by a memoir of Stout by Professor J. A. Passmore. It gives an effective summary of Stout's development and of his main philosophical views and it completes the previously published memoirs by Broad and Mace with an attractive portrayal of him in his last five years, spent in Australia. The Bibliography annexed to the Memoir is a reminder of the energy which enabled Stout to produce this impressive series of books and papers, alongside his teaching and the exacting task of editing MIND for thirty years (1891-1920).

Stout's previous book *Mind and Matter* (1931) was described as 'the first of two volumes based on the Gifford Lectures of 1919 and 1921'. The present work is the second, but it is not in any ordinary sense Vol. II. It includes the whole of the two Gifford courses with the exception of the four lectures on Materialism with which *Mind and Matter* has dealt more adequately. It therefore overlaps the other parts of *Mind and Matter*; and it requires only the addition of the refutation of Materialism from the earlier work to make it a complete piece of argument covering Stout's whole epistemological theory. It has been edited with due care and attention by the author's son, Professor A. K. Stout, from a full manuscript to which the author could never be persuaded to return. Other Gifford lecturers have been known to find it difficult to fulfil the requirement that their lectures should be published. But a thirty year interval must constitute a record.

Besides overlapping *Mind and Matter* the present volume includes versions of all Stout's philosophical (as distinct from his psychological) views. Most of these are already known to philosophers from the papers printed in *Studies in Philosophy and Psychology* (1930). What is found in *God and Nature* and nowhere else in Stout's work is a group of arguments lending support to the two 'basic presuppositions of the religious consciousness', that 'the universe is a unity in such a sense that we are justified in attempting to determine its nature as a whole, and in taking up a certain emotional and practical attitude towards it as a whole' and that 'mind is not merely something transient and incidental, but is primary and ultimate in the constitution of the universe' (p. 22). I shall call these arguments Stout's 'theistic arguments'. And, since his other views are already well-known, I shall emphasize and treat more fully these theistic arguments. But it is impossible to isolate them from their setting, and indeed all of them grow out from theories of Stout's, which his readers have met before, and

which will therefore require brief mention at each point. *Studies in Philosophy and Psychology* and *Mind and Matter* were both given full and critical reviews by Professor Broad in MIND (April 1931 and July 1932); and Stout's views on perception, universals, probability, and truth have been discussed by other writers. I shall accordingly restrict my comments to the theistic arguments and to any other topics which, so far as I know, first appear here or have not been elsewhere discussed by other writers. I shall enclose my comments in square brackets so that they need not interrupt the continuity of Stout's own argument.

The first book defines the relation of philosophy to religion and defends metaphysics against agnosticism. Philosophy must accord the claims of religious experience a critical attention. It can show that they are not necessarily invalid, and it may find a guments which tend to support them. Neither Spencerian nor scholastic agnosticism is a reasonable alternative. Spencer held that we can know that the universe exists but nothing about it; but if we have grounds for asserting its existence we have grounds (the same grounds) for inferring its nature. The scholastic view that we can know God only as we are related to him, and not as he is nor as he is related to us, is rejected because 'we cannot know how B is related to A without *pro tanto* knowing how A is related to B'. [This seems doubtful. It is true in such cases as 'B is north of A' or 'B is greater than A' but if B loves A nothing follows about how A is related to B.]

Book II defends the unity of the universe against pluralism such as Russell's (in *Problems of Philosophy*). Stout also rejects the absolutism of Hegel which overstates the logical intelligibility of the universe in claiming that the truth about the whole is deducible from the nature of a single part, and of Bradley and Bosanquet who reduce the world as we know it in space and time to unreality. From this criticism, Stout's *first theistic argument* emerges. The series of temporal events is infinite. Stout accepts Russell's assurance that there is nothing self-contradictory about infinite collections, but he argues that no infinite series can be self-complete and self-contained. 'Change and succession therefore presuppose an Eternal Being without which they are incapable of existing' (p. 51). [There is nothing further in the text or in *Mind and Matter* to explain or defend this sudden bold inference. It is made particularly difficult by Stout's insistence implied here and fully stated later (Chapter XIV) that the world of change and succession is real and not appearance, and that the Eternal Mind is distinct from it. How something essentially incomplete can retain its reality as part of the whole universe, and how the postulation of another reality of a completely different type will serve to complete it—these questions remain to puzzle the reader. If we move in this direction at all and demand self-completeness for reality, it would seem more plausible to end with a view such as that of

Bradley or McTaggart that time and change are unreal. There is the further assumption, also undefended, that the self-complete being so postulated to remedy the incompleteness of the world of change is a Mind. But is there anything essentially self-complete about a mind? The minds we know most about (our own) are essentially incomplete too. And is the Eternal Mind self-complete and self-contained without the world of change or only taken along with it? The rejection of temporal creation which immediately follows (p. 51)—though in a passage marked by Stout as in need of rewriting—makes these problems even more intractable.]

Book II continues (Chapter IV) with a criticism of Russell's pluralism which Stout regards as based wholly on his distinction between knowledge by acquaintance and knowledge by description. To know *about* x we must have prior knowledge *of* x. Description involves relations and universals. Acquaintance is with particulars. It is inexpressible and neither true nor false. These particulars, thus directly known, free from all relations, constitute Russell's pluralistic universe. Stout attacks this epistemology effectively (and of course Russell himself later modified it). There is no reason to suppose that the x we know by acquaintance is self-complete, for this presupposes without justification that we know its whole nature by acquaintance. From the fact that a relation is not part of a thing we cannot conclude that this thing could exist not so related. (Relation to the inside of something is not part of the surface, but the surface could not exist without the inside.) Knowledge by acquaintance, as Russell defines it, cannot be knowledge at all; for there can be no knowledge where nothing is known about the object and where there is no truth. Stout's own view about acquaintance (or direct awareness) is that it differs from indirect awareness not in the nature of what is known but in the kind of evidence we have. The same fact (*e.g.* that Jones has toothache) can be known directly by Jones and indirectly by someone else or by Jones himself at another time. Stout thinks that the same dangerous distinction between knowing x and knowing about x led Locke to his puzzles about substance. The only solution for the difficulties both of Locke and of Russell is to maintain that all knowledge of x is knowledge about x; and consequently that 'x itself' is simply the group of its intrinsic characteristics united in a special form of unity. But this means *all* its characteristics. The problem then arises how we can claim to have direct awareness of x when we certainly do not know directly all the characteristics of x. It was Russell's awareness of this problem which drove him to his views that the particulars we know by acquaintance are self-complete and are fully known by acquaintance. Such knowledge, he held, cannot include any knowledge of relations between characteristics which are immediately experienced and those that are not. Stout maintains on the contrary—and he rightly says this is a vital principle in all his arguments—that we

can know a whole without knowing all its parts severally, that what we know directly is known in that very experience to be partial and incomplete, that in knowing some characteristics of x directly we know something about x as a whole, and that in knowing x as a whole we know something about the wider whole to which x belongs. In memory, for example, we have an immediate experience which points beyond itself to the past. The past is not immediately known, nor is it inferred. (This crucial point recurs and is more fully illustrated later, as I note below.)

Book II closes (Chapter V) with an account of universals and particulars and of possibility and necessity, subjects on which Stout's views are already known to philosophers from Papers XIII, XV, XVI and XVII in *Studies in Philosophy and Psychology*. Embedded in this chapter, however, is a section (pp. 81-82) which is vital to his theistic conclusions. It repeats and takes further the argument of Paper VII in *Studies*. Each sensum is known as essentially incomplete and as continued beyond itself into a whole which transcends and includes it. ' This whole is apprehended as one and the same for all data '. Stout's theory of perception had included all along the view that a physical object is *known* in and through the sensing of a single sensum. But here it is asserted that a sensum points beyond itself not only to a physical object but beyond that to a single whole which is the same for all sensa. [Again there is no explanation or defence of this vital step. But presumably the suppressed argument is that the physical object to which the incomplete sensum points is in its turn incomplete and points beyond itself to a wider world of space, time, and causality; and nothing short of the whole spatio-temporal order can remedy this incompleteness; and then the spatio-temporal world is itself incomplete and requires (see the first theistic argument above) the postulation of an Eternal Mind.]

Stout considers the alternative view of Russell and others that sensa are directly *known*; but that physical objects (whether taken as entities of a different type from sensa or as groups of actual and possible sensa) are inductively inferred and therefore are matters of belief, not knowledge. He rejects this on the basis of general considerations about possibility. The Inductive Principle cannot carry the weight it must in any theory such as Russell's. Possibility and probability must rest on knowledge of necessary connexion. In the particular case in point, unless we *know* that the sensum refers to *some* physical object to whose nature the characteristics of the sensum give *some* clue, we cannot have probable belief that the object is a horse or a chair. [These general considerations are dogmatically stated and not clearly worked out, but they are already familiar from *Studies* (Paper XIV) and from *Mind and Matter* (pp. 117-120) so I shall not discuss them here.]

Book II deals with ' Matter and our Knowledge of it ' and largely overlaps *Mind and Matter* and Stout's other works on perception.

All theories which reduce the physical world to families of actual and possible sensa fail because reference to a physical object is needed to determine the set of possibilities and to give grounds for asserting them. What makes a family of sensa a family is relation to one physical object, and reference to the physical world is required to determine what sensa are possible. To reduce a chair to sensa some of which are possible—*e.g.* the sensum I should have if I were in the next room—presupposes a physical order, *e.g.* my body and the next room and the chair in it. Measurement and causal laws alike require that a physical object should have one single shape and size and position. [Some of these arguments anticipate interestingly, in their own way, certain recent criticisms of phenomenalism.] Stout holds that in sensation the physical object is known through the incompleteness of the sensum; and he goes on to claim that this knowledge of the physical object is not merely knowledge that it exists but extends to some of its characteristics. The physical object is known as 'akin in nature to the sensum' and as 'conditioning the occurrence of the sensum in the mode in which it occurs' (p. 145)—that is, as causally active.

Stout finds parallels for such knowledge through incompleteness of an immediate datum in other fields. Universals (= classes, on Stout's view) are neither immediately known nor inferred. They are known as supplementing the incompleteness of the particulars with which alone we are directly acquainted. A directly presented image points beyond itself to the previous impression of which it is a copy and which supplements its incompleteness. [This seems doubtful. It would be a more plausible line to say that when I imagine myself in a particular situation, I have an image but the image is recognizably incomplete and points beyond itself to the whole situation which I imagine through it.] In memory what is remembered is past and must be known through the incompleteness of some mode of present immediate experience. In time awareness generally, the present alone is immediately given, but is known to be incomplete as having emerged from the past and as merging into the future. Thus, all through, 'given or inferred' are not exhaustive alternatives; 'known through the incompleteness of the given' is the third alternative on which, in all these examples and particularly in the vital case of knowledge of physical objects, Stout relies.

Chapter VIII on 'External and Internal Perception' is one of the most interesting in the book. Stout gives a careful account of Introspection, Organic Sensation, Motor Sensation, and Perception. Internal perception is of two kinds; in motor sensation the order and occurrence of the sensations is within our own control (*e.g.* in willed muscular movement). In organic sensation (*e.g.* tickles, aches, itches) the occurrence and order is beyond our control. In both these cases it does not make sense to distinguish between the presented order and the order of physical occurrence. But in perception the order and occurrence of sensa is partly subject to our

will and partly not and this makes it possible to distinguish between presented order and real order of the events in question, and thus to build up our knowledge of the physical world. It is through the achievements and failures of efforts against resistance that we become aware of a world external to our bodies (and the distinction between subjective and objective succession is only a special case of this). [But are these sharp distinctions tenable ? In the case of a nervous tic, do I not have motor sensations wholly beyond my control ? When I try not to sneeze, I have an experience, sometimes successful, sometimes not, of effort against resistance. But sneezing is an example of organic (and motor ?) sensation and not a clue to an object external to my body. Nevertheless it seems to me that Stout is clearly right in the main here and that our knowledge of the external world is built up in the way he suggests.]

Stout argues that all external perception is accompanied by organic sensation. We are constantly aware of our sense-organs while using them to apprehend external objects. This is obvious in the case of touch, but Stout has considerable difficulty in justifying it over sight. How do we apprehend the eye in seeing ? [There is a mistake or misprint on p. 150 : ' We apprehend the eye as something we see in distinction from the thing seen ' ; this should read ' . . . as something *with which* we see ', as is made clear in the preceding sentence on p. 150 and again on p. 159.] How then are we ' perpetually aware of our sense-organ ' in sight ? Stout gives two answers, both of them very difficult to accept. We are aware of different parts of the eye as each seeing a different part of the visual field. We are aware of the centre as seeing more clearly, of the marginal parts as seeing more indistinctly. [But surely this is inference from the characteristics of the things seen and not direct organic sensation.] The second answer is this. Sensa occupy places in the visual field (which Stout calls ' the local sign continuum of sight '). This visual field remains constant however much the sensa vary ; it is ' a fixed system of distinguishable places which can be occupied by different sensa '. [This is reminiscent of Kant's view that we directly apprehend empty space, and seems as difficult to accept or to understand.] It was asserted above that any sensation group whose order is invariable and independent of our will is an organic sensation group. Hence, since the order of ' places ' in the local sign continuum is invariable it is inferred that it is apprehended by organic sensation yielding knowledge of the body of the percipient. Since the continuum is visual the part of the body so apprehended must be the seeing eye.

Chapters IX and X go on to give a careful analysis of the process by which we determine the nature of the physical world, by distinguishing empirically and experimentally conditions due to sense-organ, medium, and object. The principle of causality is presupposed throughout these empirical processes and is itself a primary datum. [Compare Whitehead's similar view on this point.]

Our primitive and basic awareness of our sensum includes knowledge that is caused by a physical object akin to it in nature: but this does not determine in detail the characteristics of this object. This is done by induction with one single exception. In the case of physical effects due to conscious effort we are aware of a connexion between the effort and the effect. This is not a necessary connexion between the two in isolation. We are aware that the effort *can* be a factor in the total cause of the effect and *will* be so if certain other factors are present. Our other source of determination of the detailed nature of a physical object is measurement, and in the last resort measurement by superposition, of which Stout gives a complicated and ingenious analysis. He accepts the reality of secondary qualities and he holds that the real colours are those seen by a normal eye in ordinary daylight and not those seen in twilight or by the colour-blind. [' Normal ' is a difficult word here especially if, as seems likely, it means only ' average '. Anyone who wishes to hold that some seen colours are real and others not would be better advised to hold that the real colour is that to which approximation is made by the eye or the light or the instrument which discriminates most consistently between the greatest variety of shades. This is in fact the principle Stout himself implicitly adopts when he admits that colours seen through a microscope are real though *not* revealed to ' normal eyes in ordinary daylight '. It is the principle implied in tests for colour-blindness, but it leads to the admission that artists and dress designers may meet such tests better than ordinary men with ' normal ' eyes. And ' ordinary daylight ' would cover anything from Mentone to Manchester. Any artist will tell us how rare a ' good light ' is and his principle for deciding which light is good is again the principle of maximum differentiation stated above.]

Chapter XI returns to the nature of sensa. Sensa are continuous with and akin to physical objects and are therefore material not mental in character. They are parts of matter but not parts of physical objects; they are the parts of matter directly presented to mind.

Book IV on ' The Universal Correlation of Mind and Matter ' uses the conclusions established in Book III to reach general metaphysical results. Stout holds, in agreement with Berkeley and against Alexander and an early view of Moore, that no sensum could exist unperceived. For some sensa (organic) cannot exist unperceived and no line can easily be drawn between them and the others, especially if we accept Ward's view that all sensa arise from differentiation of a relatively homogeneous presentation-continuum. All sensa then are mind-dependent. But sensa are parts of matter, so matter is mind-dependent also. (This conclusion is fortified by the argument of *Mind and Matter* that mind cannot be matter-dependent but can be derived only from mind.) But does matter depend on mind or on minds? Do finite minds

arise from other finite minds (which would involve a monadistic metaphysic), or from mental existence below the level of finite minds (a mind-stuff metaphysic), or from universal mind? Monadism is inadequate because of its denial of relations (and anyhow it requires a Universal Mind for its pre-established harmony). Mind-stuff theories appeal to scientists because of their atomistic analogies, but no intelligible theory of combination or interaction of such mental atoms can account for the unity of a personality. There remains the hypothesis of a Universal Mind—and this is Stout's *second theistic argument.* [It is not clear whether the argument is that sensa depend on finite minds and finite minds on Universal Mind, or that sensa themselves, or Matter, depend on Universal Mind. If the former is intended this section adds no theistic force to the argument of *Mind and Matter.* But if, as seems more likely, the latter is the meaning, there are serious difficulties. All the positive evidence for mind-dependence of sensa is for dependence on finite minds. Is the suggestion that those features of sensa not dependent on finite minds depend on Universal Mind (Berkeley's view) or that the parts of matter other than sensa depend on Universal Mind also ? There is no argument for either conclusion, and at least the first involves attributing sensation to God (as Stout admits on p. 241) and further difficulties in reconciling the continuity Stout defends between sensum, matter, and physical object with any distinction between dependence on finite and dependence on infinite Mind.]

Chapter XIV analyses the conception of a universal mind and shows it to be compatible with the reality of finite minds (against Bradley's arguments).

Chapter XV discusses the correlation of the mind-matter and mind-body problems and concludes that the mind-matter problem is basic. The mind-body problem so far as it concerns epistemology is the problem of the relation of sensa to brain-states. But we have seen that sensa are parts of matter so this is a problem *within* the material world. The facts of reproduction, retentiveness and association require the continuance of sensa or of their conditions in the world of matter including perhaps (though not necessarily) in that part of matter we know as the brain. But material conditions alone will not account for these facts, for they involve cognitive, conative and affective dispositions. Here we require mind as well as matter to provide the continuity involved, and the mind required must be the universal mind. This is Stout's *third theistic argument.* Psycho-physical dispositions as continuing elements in our mental lives are inexplicable without reference to a single universal mind. [Again there seems to be a gap in the argument. Why should not the continuity be given by finite minds ? And it is difficult to understand how a universal mind which is eternal can explain dispositions which, while enduring, are not eternal but have a beginning and an end. Nevertheless,

against purely materialistic or brain-trace theories the argument has force, and the alternative theory (of finite minds) usually involves the notion of the unconscious which has difficulties of its own.] This ends the argument for the existence of a universal mind based on our knowledge of matter.

Book V deals with our knowledge of mind. Our own minds are known to us neither directly nor by inference but (as before, the third alternative) in knowing subjective states as essentially incomplete; 'pleasure and desire reveal themselves as phases in the life-history of a subject'. [This section is tantalizingly brief and sketchy, especially from Stout.] How do we know about other minds? Not by inference from bodily behaviour: this would mean that we know our own minds directly but other minds only through resemblances between our bodies and theirs. In fact, however, 'there is a primary demand for psychical existence beyond the individual self'. That this psychical existence takes the form, in certain cases, of finite minds like our own is indeed an inference based on bodily behaviour and on responsiveness to practical action. But this tentative inference works from and within a more basic certainty that there is mind beyond our own in the world. [Here is another example of a type of argument frequently found in Stout. It is perhaps to be traced to Kant whose influence on Stout is noticeable throughout. Hume had distinguished two questions: (*a*) Why do we say every event has a cause?—the general principle, (*b*) Why do we say this event has this particular cause?—the particular causal law—*e.g.* that microbes cause disease. Hume 'found it convenient to sink' question (*a*) in question (*b*)—where it remained sunk without trace—and answered question (*b*) by the theory of induction. He thought that 'perhaps the same answer will serve for both questions'. Kant accepts Hume's answer to question (*b*) but does not think it will serve for question (*a*). 'Special laws . . . cannot in their specific character be *derived* from the categories though they are one and all subject to them. To obtain any knowledge of these special laws we must resort to experience, but it is the *a priori* laws alone which can instruct us in regard to experience in general and as to what it is that can be known as an object of experience' (*Critique of Pure Reason*, Ed. II, p. 165; Kemp Smith's translation p. 173). So with causality, Stout maintains that we are directly aware of a single sense datum as having been caused by something continuous with and akin to it; within this basic and certain knowledge we proceed to infer, fallibly and inductively, that the cause is a horse or a chair.]

In support of this vital step in his argument Stout urges the following points: (*a*) if I were primarily aware only of my own mind I could never think of other minds as even possible nor of my own mind as *mine* ('only my own' implies the possibility of others) (*b*) primitive animism certainly does not rest its belief in other

minds on analogy with human bodies or on responsiveness of a type similar to our own actions. So such inference cannot be basic to our awareness of other minds; [this is a strong and interesting point]; (*c*) even now we treat things as active individuals; the concept of *force*, never observable in nature and useless to science, is ineradicable from our ordinary thought; (*d*) in aesthetic experience we find mind attributed to nature and not merely as a play of the imagination or by the pathetic fallacy; at its most sublime, *e.g.* in Wordsworth, this aesthetic experience of a mind behind things borders on religion; (*e*) religion in its turn claims to carry conviction of a mind behind all phenomena. If it is objected that God can be explained as an anthropomorphism [or as a father-substitute —though Stout does not add this] the answer is that anthropomorphism explains only the crudities, the errors, the superstitions, and the heresies in religion. The most developed religions are freest from it [and from father-substitute elements also]. These points taken together may be held to constitute Stout's *fourth theistic argument*—that in our awareness of the external world we are aware of a mind other than our own and mind not exhausted by those finite minds for which at any given time we happen to have empirical evidence.

Stout finds a *fifth theistic argument* in a restated form of the argument from design. This section (like the corresponding section in *Mind and Matter*) is the least convincing in the book. Nature is said to reveal a teleological order as well as a causal order. [But Stout completely fails to give any clear meaning to 'teleological'. 'Complexity', 'system', and 'perfection' are all terms used to explain it. 'Perfection' is obviously a question-begging term, and 'system' seems to mean no more than complexity. 'Adaptation to a purpose' is certainly not meant, as is shown by the example of the structure of chemical molecules. The carbon compounds in particular show teleological order in a very high degree. Then having used 'teleological' in this very Pickwickian sense, he seems to take advantage of its ordinary sense to imply a design and a designer, as for example when he argues that 'existing teleological arrangements presuppose prior teleological arrangements adapted to account for them'. 'Adapted' and 'to account for' seem to slip in design, unless he is prepared to rewrite the sentence to read 'the causal conditions of a complex whole must be at least as complex as the whole to which they give rise', which is a doubtful proposition in itself and is in any case in no sense a rehabilitation of the argument from design.] At the end of this section, however, a new and more plausible argument—the *sixth theistic argument*—emerges, though it rests at a crucial point on one of the most debatable conclusions of *Mind and Matter*. The argument is this: (*a*) Particular teleological systems (such as watches) which are products of human will indicate a prior teleological system (of thoughts and desires) in the mind of the maker. (*b*) But individual minds

are not isolated teleological systems. Life is a complex transaction between individual and environment; the mind of the watchmaker is only one set of conditions among others of almost equal complexity which are required for the production of a watch. So the total set of conditions must be grasped as a whole if we are to understand the making of a watch. Now this whole teleological system created by the interlocking of individual minds with environment can be explained in two ways. Either the system as a whole is evidence of the presence of universal mind, or the system as a whole is material and some of these material conditions give rise here and there to the sub-systems we call finite minds. But the second solution, epiphenomenalism (or 'materialism' in Stout's usage), has been refuted in *Mind and Matter*. So only the first remains; and in support of this it may be pointed out that teleology belongs essentially to mind as such—a mind which was not a teleological system would not be a mind. But matter may or may not be teleologically organized.

The last sections of the book (Chapters XVII to XXI) argue that the unity of the mind implies the unity of the universe. This is argued first (Chapter XVII) in connexion with cognition. The unity of the mind in knowing presupposes the systematic unity of the universe known. This Kantian argument is combined (not without difficulty) with the non-Kantian view that the knowing mind does not determine the structure of what is known. The argument is that ignorance is always partial. What we do not know we always to some extent do know as connected with what we know already. If there were two experiences between which there could be no knowable relation this would be equivalent to saying the two experiences must belong to different minds. For experiences to be related, their objects must be related too. If I were aware of two universes between which there were no intelligible relations, I should not be a single cognitive self. And indeed it is just so far as, in extreme cases, some people approach this condition that we have to speak of schizophrenia or dissociated personality. Thus, for me to have achieved a (relatively) unified cognitive self I must know a (partially) unified universe. And so far as my ignorance itself is, as it must be, the partial awareness of other conditions intelligibly linked with what I know already, I have (potentially) a fully unified cognitive mind, but this potentiality presupposes a fully unified universe. [The argument hereabouts seems to turn a great deal on the word 'know' and the chapter ends with a section (pp. 272-280) on this subject. 'Knowing' is neither an act nor a state of mind nor a relation between a mind and its objects. But I have found it quite impossible to discover Stout's positive view of what knowing is.] Knowing and being then are essentially and inseparably united aspects of the whole universe. [Stout is here, I think, converting the proposition, which he has so far proved, that the unity of the mind implies the unity of the

universe, and suggesting that the unity of the universe implies a single universal mind. But this conversion is undefended.] It follows that there must always have been knowing minds or mind. 'Minds' are ruled out by the anti-monadistic arguments of the earlier chapters : and how could finite minds know the given as incomplete and as ultimately implying the whole of nature unless they had their source in infinite mind. Here is the *seventh theistic argument.* [It is an idealist argument familiar in Green and Bosanquet and I shall not comment on it further.]

Finally we have the *eighth theistic argument,* said by Stout (p. 288) to be so important that all the rest of the book is but preparatory to it. This is the argument from the unity of interest found in the practical and moral life to the unity of the universe. (Chapters XIX and XXI—Chapter XX is a digression on dissociations.) Unity of interest is low in animals and children whose desires operate successively and independently of each other. It is higher in lives such as those of Scrooge or Macbeth which are unified in the service of some particular and limited purpose—wealth or ambition. But, as they did both in Scrooge and in Macbeth, the suppressed desires keep revolting against such monomaniac control. Only on the highest levels of morality (in Wordsworth's 'happy warrior') or religion (in the lives of the saints) do we find real unity of interest, that unity of which happiness and peace of mind are the sure symptoms. There is no final satisfaction in any finite good. If we aim to satisfy individual desires as they come, failure is certain ; we all know 'the vanity of human wishes'. If 'good' meant simply what is desired, as Hobbes held, this pessimism would be the last word. Stout invokes the parallel of truth. If truth was merely what happened to be believed, there would be countless truths contradicting each other. But truth is objective. So too with good. Good is what ought to be desired and this is independent of actual desire. Value belongs to the constitution of the universe. We can thus justifiably argue that an ideally unified self is possible only in relation to an ideally unified objective good. For human beings the goal is unattainable ; and so, in another sense, failure is certain. (A man who is proud of his human success reveals the limitations of his aim and the shallowness of his mind.) But it is a failure which is aware of its direction and which does not tarnish the achievement in that direction which it accompanies. And the men who are most acutely aware of it are also, by their very humility, witnesses to the reality of their ideals, and themselves vividly aware of the universe as a moral universe. [This culminating argument is very briefly and not always clearly stated. Nevertheless there seems to be considerable force in its main contention. At the heights of moral experience men do seem to have been vividly aware of being at home in the universe. If the universe were really materialistic should we not, as clear-sighted realists, eat drink and be merry ? If it is pluralistic, should we not

recognize that children and animals in their pursuit of separate isolated goods have wills most closely akin to the principles of nature? Life should be like reality, just one damn thing after another. Unless the universe is a moral universe, we must admit that the higher the moral life ascends the greater the illusions it involves; and we shall find it hard to explain how any being which, like all beings, is merely finite could ever have such illusions at all.]

[It is of course difficult to review such a book as this at a time when the general atmosphere of philosophy has become so rarefied that no metaphysical argument can survive in it; and the whole work will make little appeal to those who know *a priori* that this sort of thing just cannot be done. But even they, if they will avert their eyes from words such as 'God', 'nature', 'whole', and 'universe', will find here incidental discussions well worth careful consideration. I refer particularly to the sections on external and internal perception (Chapter VIII), on measurement (Chapter X), on brain and mind (Chapter XV), and on conflict and dissociation (Chapter XX).

I have treated the argument, as it was meant by its author to be treated, as a metaphysical alternative to other metaphysics. In this capacity, as against materialism, monadism and mind-stuff theories, it seems to me to make a strong case. As for its own special contribution, the impression it leaves in the end is that all the eight theistic arguments, despite gaps and obscurities in some of them, deserve careful scrutiny and that the fourth and the eighth particularly do go some way to support the claims of religious experience. The effect of selecting the theistic arguments for emphasis and discussion in this review has inevitably been to focus attention on the most difficult and debatable sections of the work; and this has suggested a degree of doubt and disagreement which is out of proportion to the truth.

It may well be that the present fashion in philosophy is only one of those recurrent anti-metaphysical phases of which the history of philosophy is full. In that case *God and Nature* should share in any revival of interest in ultimate questions.]

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Introduction to Mathematical Thinking. By DR. FRIEDRICH WAISMANN: translated by THEODORE J. BENAC: foreword by KARL MENGER. Hafner Publishing Company, London, 1951. Pp. xii + 260.

DR. WAISMANN'S book aims at setting out for a reader fresh to the philosophy of mathematics the principal results which he regards as established and opinions he considers sound. The book, originally published in 1936 (a fact which is not noted in this translation),

constitutes a forceful exposition of a general view of the subject with which I do not agree, and I shall therefore criticise certain fundamental points: moreover, in a book designed as an introduction, Dr. Waismann might perhaps have indicated more often and less tendentiously the possibilities of alternative approaches. But aside from this, the book achieves its aim. It is lucid and orderly; the chapters on such non-controversial matters as the concepts of "limit" and "curve" are admirable, and there are many illuminating historical observations. The rarity of good introductory books on this subject makes it especially welcome.

The book will also interest those already acquainted with the subject for the arguments for Dr. Waismann's views on disputed questions, and particularly for the reports of Wittgenstein's opinions. Dr. Waismann looks on arithmetic as a formal calculus based on arbitrary conventions, but closely related to our actual method of using number-words in ordinary speech—a use which is also based on arbitrary conventions and which might have been different.

Anti-formalists do not object to the very concept of an uninterpreted calculus: however puzzling this concept, it certainly has application. They question rather the capacity of formalism to account properly for certain features of mathematics, for example infinity, mathematical existence, or statements of number inside mathematics (*e.g.* "the *number* of primes less than a given number"). I do not intend here to discuss this whole question: what I wish to point out is how different certain true statements about meaningful arithmetic look when transferred to the context of formalist arithmetic.

Dr. Waismann lays great stress on a point upon which Russell has also vehemently insisted—namely that the integer $+2$ and the rational $2/1$ are not to be identified with the natural number 2: that in introducing negative numbers or fractions we do not add new numbers to an already existing set, but enter a wholly new domain, a subset of which is isomorphic with the previously recognised system: that by introducing them we do not solve the equations "$x + 3 = 2$" and "$3x = 2$", but merely the analogous equations "$x + (+3) = +2$" and "$(3/1)x = 2/1$". But this assertion—which I shall call the "Russell point"—has a very different significance when made by a fellow-traveller of the formalists from that which it had when made by the anti-formalist Russell, although the difference is obscured by the order of Dr. Waismann's chapters.

Dr. Waismann first sets out the class-room view that the system of numbers is progressively extended by the addition of new types of numbers. He then raises objections to this idea, and goes on to define the integers as couples of natural numbers and the rationals as couples of integers. For these couples definitions of "sum", "product", "equality", etc., are chosen which correspond with truths about natural-number differences and quotients, *when these*

exist, and which make the properties of these operations and relations correspond to those of their natural-number analogues. The Russell point is then made at length.

Now why should it be necessary to *define* the rationals and the integers in terms of the natural numbers ? Someone might suggest, " In order to show the consistency of the calculi of rationals and integers relative to that of the natural numbers ". But definitions are not necessary for this, merely the construction of a model—a procedure which Dr. Waismann ably describes in connexion with Klein's proof of the consistency of hyperbolic relative to Euclidean geometry : we do not *define* the former in terms of the latter, but merely show that a part of the latter is isomorphic with the former. One would, rather, naturally assume that the definitions of the rationals in terms of the integers and the integers in terms of the natural numbers were a prelude to defining the natural numbers themselves. But this is not what Dr. Waismann does : when he comes to deal with the natural numbers he merely sets up a formal calculus based on a set of axioms, from which various theorems are proved. It is true that he avoids taking up a pure formalist position, since he holds that this calculus provides a method of representing some valid inferences of ordinary language. Exactly how it does this we are not however told : there is no question of making the calculus meaningful by providing a precise interpretation. And we are expressly told that mathematics contains many other calculi which similarly represent other patterns of valid inference.

Why then was it necessary to *define* the rationals and the integers? Would it not have been simpler—or at least just as adequate—to set up further formal calculi, one for the integers and one for the rationals ? How is it we have been led into a position in which it seemed to be *necessary* to define the rationals and the integers ?

At this point it must strike one as important to look more closely at the objections to the class-room view, since it was because this view was said to be untenable that we were urged to admit the necessity for the definitions. Now Dr. Waismann brings only two solid objections to the view, both contained in quotations, one from Russell and one from Frege. That from Russell is the famous one about the advantages of theft over honest toil. This is a perfectly valid objection to the idea of ' postulating ' numbers which should provide solutions to hitherto insoluble equations : but while formalists are in practice more given to ' postulating ' than other people, it is an objection which a formalist as such could endorse as readily as anyone else. The setting up of a formal calculus is as much honest toil as providing definitions ; the formalist might rightly say that ' postulating ' is no use : one has to provide the formal rules for manipulation. So far, therefore, nothing has been suggested which impels us to seek *definitions* of the integers or the rationals.

XUM

The other quotation is from the *Grundlagen* (p. 112), where Frege asks why we do not assume the existence of numbers to provide solutions for equations like "$1/0 = x$" or "$1^x = 2$". He says that someone would reply that such an assumption contains a contradiction, and answers that in that case the assumption that there exist solutions to "$3 + x = 2$" and "$3x = 2$" might also contain a contradiction. (I have substituted Dr. Waismann's examples for Frege's). Dr. Waismann uses this quotation to show that the consistency of the rational and integer calculi must be established. But this, though desirable, has no tendency to make us wish to *define* the integers and rationals. I am not sure exactly what Frege's meaning was in this passage: but the point surely is that there is just as much of a contradiction in supposing the existence of a solution to "$3x = 2$" (where 3 and 2 are natural numbers) as to "$1^x = 2$". Now Dr. Waismann might reply that he has taken this into account by stressing the Russell point. But he is able to do this so forcibly only because he has defined the integers and rationals in terms of the natural numbers: he has shown no *reason* for giving such definitions.

Suppose that Dr. Waismann had not provided these definitions, but had instead simply set up formal calculi for the rationals and the integers: by such a procedure all the objections he makes to the class-room view would be met. What then would become of his insistence, with Russell, on the non-identity of 2, + 2 and 2/1? Someone who regarded + 2 as identical with 2 might say that the calculus of integers was a mere extension of the calculus of natural numbers. To this Dr. Waismann's only reply would be, "You cannot *extend* a calculus, for a calculus is something timeless: if you try to extend a calculus, you merely get a new calculus"; for the *only* compelling reason for the formalist for saying that + 2 is not identical with 2 is that they are elements in different calculi.

Now it is very important that this is *not* the only reason for an anti-formalist to assert the non-identity of + 2 and 2. It is *not*, as Dr. Waismann says, simply that there is no natural number which added to 7 gives 5: it is that there is *nothing whatever* which added to 7 gives 5 (7 and 5 being natural numbers, and "+" being interpreted as defined for natural numbers). By contrast, although there is no *natural number* x such that $2 + x = x$, there *are things* of this sort, for example $\aleph_0$. And this is why it is customary to distinguish positive integers from natural numbers, but to identify finite cardinals with natural numbers. But for the formalist there is no difference between the two cases: the calculus of cardinals is as different from the calculus of natural numbers as is that of integers; if one cannot extend the calculus of natural numbers by introducing negative numbers, one cannot extend it by introducing transfinites either.

Moreover, someone might argue that, historically, 0 was recognised as a number later than the other natural numbers, and that

by introducing it the calculus was changed—and, indeed, the change in the calculus is obvious and marked: so that the natural number 2 must also be distinguished from the number 2 which is an element of a calculus of which 0 is not an element. And might not someone wish to say the same, not only of 0, but of 1 and even of 2? So that one might find oneself forced to distinguish between the multiple number 3, the plural number 3, the counting number 3 and the natural number 3 before one ever reached the integer 3. Now Dr. Waismann also stresses this kind of possibility, but from very different motives. He points out that one can find languages which, beyond 5, have the 'number' *many*; languages in which there is no word "nought"; languages in which the numerals "1"–"4" function in grammatically different ways from the succeeding numerals; and so on: and he says that all these languages suggest very different calculi from the calculus of natural numbers which we have adopted. His object here is to discredit the apparent sacrosanctity of the calculus of natural numbers which the idea of deriving it from logic gives it: for here are quite different but in certain ways equally plausible calculi. Now in one sense these languages obviously would provide us with quite different calculi: but as the *only* reason which Dr. Waismann can ultimately give for distinguishing the natural number 3 from the integer 3 and the rational 3 is that they are elements in different calculi, he would have, to be consistent, to admit that "3" in *these* calculi had different meanings—stood for different numbers—from that which it has in our natural-number calculus. Whereas Russell, in distinguishing + 3 from 3, was referring to something quite different: for he would have said that "3" in these languages nevertheless *meant just the same* as it does in ours. Of course "means the same" is ambiguous: sometimes identity of application may be reckoned a sufficient condition of synonymy, sometimes one may feel inclined to take into account the concepts which surround the one in question. But the point is that where Russell, in distinguishing the meanings of "3" and "+ 3", was concerned only with differences in definitions designed simply to set out the conditions of application, Dr. Waismann, as a near-formalist, can take account only of what surrounds the manipulation of the symbols in question—the calculi of which they are part; and hence the same assertion in Dr. Waismann's mouth has a very different sense from that which it had when Russell made it. For Russell would say that "3" in these languages had the same meaning as in ours, because its application was the same, *i.e.* because the same definition could be given of it, whether 0 and 1 (and for that matter $\aleph_0$) were regarded as elements of the calculus of which it was a part or not. This attitude gives a point to Frege's assertion that 0 and 1 are just as much numbers as 2,378: to attempt to overturn this thesis by referring to languages in which there is no noun "nought", or in which the only number-words are "none", "one" and "many", or "none"

and "some", is simply to miss the point. Not that Dr. Waismann does this: but it is an example of the kind of indifferentism to which his outlook can easily lead.

I have laboured this point because I think it is important to see how different such statements look when made by the formalist, and how easy it is for the formalist to borrow from meaningful arithmetic procedures (like those of defining the integers and rationals) which he has no reason to employ, and so to conceal the extent of his divergence from his opponents. But Dr. Waismann has not even been consistent: for right in the middle of the chapter on the rationals comes the suggestion that certain considerations might lead one to add ∞ to the real numbers, to abandon 0, and to set $-2 > -1 > \infty$. It is true indeed that one might do this, but on Dr. Waismann's own principles this would be setting up a radically new calculus, and he has no right to refer here to "real numbers". Similarly, later in the book, after pointing out very rightly that in talking about limits we need only the 'potential-infinite' not the 'actual-infinite', he subsequently says that we could incorporate the 'improper' number ∞ into our number-system, and hence count unboundedly increasing sequences among convergent ones. This must be extremely confusing for the beginner for whom the book is designed: and it is confusing precisely because Dr. Waismann refuses to ask the question which for the anti-formalist would be crucial, "Would incorporating ∞ involve altering the definitions we have already got?" Just because it is for such comparatively trivial reasons that Dr. Waismann distinguishes between 3 and $+3$, he feels that having once made the point emphatically it may for convenience be subsequently ignored.

I shall now discuss Dr. Waismann's views on equality. It will have been noted that "$=$" was among the symbols which Dr. Waismann defined afresh for the integers and the rationals. This is not the usual procedure: normally "$=$" is the one symbol whose meaning is regarded as remaining constant as we pass from one domain of numbers to another; for since Frege the sign has been interpreted in all contexts as meaning "is identical with". I do not wish however to criticise Dr. Waismann simply for departing from the usual conception, for there are certain clear advantages in doing so. But it involves also difficulties of its own. Dr. Waismann does not explain that he is doing anything unusual; he stresses however some of the advantages, but slides over the disadvantages. Having defined integers as couples of natural numbers, not as *classes* of such couples, and rationals as couples of integers, not as *classes* of such couples, he then proceeds to speak repeatedly of *the* null-couple, and of *particular forms* of number-couples, in spite of the fact that in both cases there will be an infinity of null-couples, and that what he calls "particular forms" of number-couples will be *different* number-couples all equal to one another. This is particularly objectionable in view of Dr.

Waismann's claim that the integers and rationals can be defined in terms of the natural numbers with the aid only of a notion so little abstruse as that of an ordered pair, whereas to justify the mode of expression he later uses he would need also that of an infinite class; and in view of his distinction between the methods of defining real numbers and rationals or integers, that the former but not the latter involve infinity and the notion of a class; whereas Russell's version of Dedekind's definition of real numbers makes them classes of rationals, and the ordinary logistic definitions of rationals and integers make these relations between (classes of couples of) integers and natural numbers respectively.

I am not denying the validity of Dr. Waismann's definitions of the rationals and the integers, or the corresponding definitions of equality. After all, the relation which Dr. Waismann calls "equality" does appear in the logistic system, namely as membership of the same integer- or rational number-class, so that Dr. Waismann may reasonably claim to be effecting an economy; and statements of the form, "There are two rational numbers which . . ." can easily be rendered in Dr. Waismann's system in the form, "There are two *unequal* rational numbers which . . .". All I have to quarrel with is Dr. Waismann's borrowing features of the usual definitions when it happens to suit his purpose. Similarly, I do not dispute that the step from the rationals to the reals—leading as it does to numbers transcendental over the rationals—is different in kind from that from the natural numbers to the integers, or from the integers to the rationals: but the possibility of Russell's methods of taking these three steps makes it implausible to account for the difference in this simple manner.

But Dr. Waismann's method of treating equality has disadvantages of its own. For (1) in the field of complex numbers Dr. Waismann treats "$=$" as meaning "is identical with", and for reals he thinks it possible to do so (Dedekind's theory). Now his only requirements for an interpretation of "$=$" are that it shall be reflexive, symmetrical and transitive. (Though even of these he is prepared, on Wittgenstein's authority, to relax the last; I cannot myself feel that the authority of any philosopher, however great, can permit us to treat language quite so Humpty-Dumptyishly.) But while identity of course fulfils these requirements, the arguments seem to me overwhelming against regarding identity as a genuine relation: whereas equality between integers, or between rationals, as defined by Dr. Waismann, *is* a perfectly genuine relation. This difference seems so marked that it cannot be right to use the same sign to stand in different contexts for identity and for equality: despite the transitivity and so on, analogy cannot be made to stretch that far; besides, the use of "$=$" as a sign of identity cannot be restricted to certain particular domains. Of course, it is possible to devise definitions of both real and complex numbers which allow different ones to be equal: but if we do this, we have

clearly purchased elegance for the integers and rationals at the expense of elegance for the real and complex numbers.

To elaborate this point a little.[1] If there *is* to be a sign meaning "is the same as" in general, then there are strong reasons for reverting to Frege's *Begriffschrift* view, that in "$a = b$", the names "a" and "b" are not *used*, but *mentioned*; "$a = b$" does not say of the *object* of which "a" and "b" are names that it is identical with itself: it says of the *names* that they are names of the same object. On these grounds it would be preferable to replace the notation "$a = b$" by something like "'a' $\overline{=}_{\text{df}}$ 'b'". But then one cannot similarly replace the matrix "$x = y$" by "'x' $\overline{=}_{\text{df}}$ 'y'": for "'x'" and "'y'" are not variables ranging over names, but names of variables; and as "'x'" and "'y'" are not variables, "'x' $\overline{=}_{\text{df}}$ 'y'" is not a matrix but a sentence, and a meaningless sentence, because as "x" and "y" are not names of anything, they cannot be names of the same thing. Now this might be a good reason for abandoning identity altogether in logic, and interpreting equality otherwise than as identity. But this course should lead us to give such another interpretation of equality *throughout*, and not, as Dr. Waismann does, fall back on the traditional interpretation when it proves simpler, as for the real and complex numbers. But doubts arise whether it would be possible to carry such a programme through—to expel identity from logic. For example, it seems necessary to make equality, in its new interpretation, a reflexive relation. But without identity, how can one stipulate this? One could of course adopt it as an axiom, in the form, "$(x) . x \doteq x$": but one wishes to avoid unnecessary axioms, and put as much as one can into definitions; and it appears as though the definition must start off "$x \doteq y : \equiv_{\text{def}} : x = y . \text{v} . \ldots$". It may be said that the whole trouble might be avoided by not specifying that "$\doteq$" is reflexive, but by considerably complicating the expression of a large number of mathematical statements: but it is not clear that this could be done without again bringing in identity, and in any case Dr. Waismann's claim that his method of interpreting equality leads to an increase in elegance collapses.

(2) Interpreting "$=$" otherwise than as meaning "is identical with" destroys the ordering of the integers (or of the rationals): for if *different* integers (or rationals), *i.e.* number-couples, may be equal to each other, the relation "$>$" is not connected in this class. This may not be regarded as a serious matter, but it should be faced. Dr. Waismann on the contrary explicitly asserts the contradictory (p. 33): but he does this only by once more appealing surreptitiously to the usual interpretation of "$=$" as "is identical with".

[1] In this paragraph I use "$=$" to signify *only* identity, and "$\doteq$" to signify equality *as opposed to* identity.

I have commented on the first half of Dr. Waismann's book at length, and have no space for a full discussion of the remaining equally interesting sections: I will therefore confine myself to a brief summary with occasional marginal comments. Chapter VIII contains a report of Wittgenstein's views on finite induction. (*cf.* Langford's review in the *Journal of Symbolic Logic*.) There follows a discussion of the foundations in which formalism as such is criticised on the basis of Gödel's famous result, and that of Skolem that no finite set of axioms could completely characterise the series of natural numbers. The logistic school is then subjected to a wholesale onslaught. The main criticisms are: one attributed to Wittgenstein, that to say that there are just as many F's as G's is not to say that there *is* a relation which sets up a one-to-one correspondence between the F's and the G's, but simply that there logically *could* be such a relation, but that if there *is* not such a relation, one can determine whether there could be only by first determining whether there were just as many F's as G's; secondly, that Frege's definitions do not take account of the use of numerals in commands; and finally, the difficulty about the axiom of infinity. The first objection overlooks the very extended sense in which "relation" is used in Russellian logic, namely as that for which *any* two-place predicate stands, whether this would be rendered in English by a relational expression, or a conjunction of one-place predicates, or in any other way. If anyone wishes to dispute the use of the word "relation" in this way, he is at liberty to, but this cannot affect the definition of numerical equivalence. The only way in which to deny the validity of the definition is to deny the identity of indiscernibles; in Russell's extended sense of "relation", given the truth of this principle, if two sets are numerically equivalent, there must actually *be* a relation which sets up a one-to-one correspondence, and even if one accepts Dr. Waismann's peremptory observation that "counting must not be regarded as a mapping of two sets by a relation" (why not?), one could establish that there was such a relation without begging the question. The second criticism neglects Frege's distinction between assertion and content. Frege says that the same content may be the content of a judgment, of a hypothesis and of a question: surely also of a command. As Mr. Hare has suggested, we could use *one* sign in *all* commands, as Frege uses the assertion sign in *all* assertions; to this sign would be attached an expression neutral as to the type of utterance it was used to make. There may be arguments against this picture of language; but Frege *did* have a way of dealing with the occurrence of numerals in such utterances as commands. Dr. Waismann does give a rather sketchy account of the theory of types; but it is rather unfair of him to criticise the "Frege-Russell theory" by reference to the axiom of infinity without mentioning that this is needed only if one adopts the theory of types, and that Frege did not do so. (Indeed, Dr. Waismann is often unfair to

Frege : as when he says (p. 71) that Frege made no attempt to solve the Russell paradox, or (same page) that he thought logic was about the *objects* (instead of *functions*) which " and " and " if " stand for.)

There follow excellent expositions of the concepts of limit and curve, of the foundation of the differential calculus, of Cantor's and Dedekind's methods of defining real numbers, and of complex numbers. (Though the impression is given that the reason why the complex numbers do not form an ordered field is that no relation has been defined which simply orders them : in fact, there is such a relation, but the requirements for a field's being called " ordered " are more stringent than this, and the complex numbers do not satisfy them). Finally, there is a chapter asserting against Frege that progress in mathematics is invention, not discovery. This discussion is interesting ; I have not however space to say more about it save that in the argument against Frege, attributed to Wittgenstein, that if mathematical symbols express a sense, that is, a mental process, we ought to be able to describe this mental process—this " state of mind "—without using the symbols, something must have gone wrong with both attributions ; Wittgenstein must surely have been aware that psychologism was one of the chief objects of Frege's attacks.

A final point. Dr. Waismann argues that, if he glanced at the sky, on the logistic theory it would seem to be necessary that there was some one definite number which was the number of stars he saw in that glance : whereas, he says, it would not be sense to say that there were 7,539—only that there were very many. Whether Dr. Waismann is right or not about what would be sense here, he is under a misconception in thinking this a criticism of the logistic view. That there must be some one definite number which is the number of stars which Dr. Waismann saw, is held by Frege and Russell to be true provided that of each individual star it is either true or false that Dr. Waismann saw it. This was brought out very clearly by Frege, who said that he considered only concepts with determinate extensions. Only if there were some star of which it was neither true nor false that Dr. Waismann saw it in that glance, could there be no such definite number ; and in such a case *star that Dr. Waismann saw* becomes a concept with indeterminate extension. With such concepts neither Frege nor Russell claims to deal.

There are two incidental blunders. On p. 40 the Diophantine equation " $3x = 2y + 4$ " is said to have no solution : whereas it has an infinity of solutions of the form " $x = 2a + 2, y = 3a + 1$ ". On p. 115 it is stated that if there were only nine individuals in the world, on Russell's definitions all the natural numbers from 10 up would be identical with one another, and with 0 : they would of course be identical with one another and with the null class of classes, but not with 0, which is the unit class of the null class.

The translation, which has not been revised by Dr. Waismann, is

of very poor quality. A simple evidence of this fact is that the Russell quotation about theft and honest toil is retranslated from the German, and mistranslated; it is also surprising that Professor Benac should claim on pp. 105 and 106 to have translated from the Norwegian the titles of two articles by Skolem published in Norway but written in German. The standard of printing is also low. Here is a list of the most glaring of the other misprints and mistranslations:

p. 3, l. 23: read "it is not in general true that" for "in general".
p. 12, l. 24: read "problem of subtracting" for "proposition to subtract".
p. 33, l. 3: read "shall also hold" for "shall also be valid".
p. 84, l. 26: read "irreflexive" for "inflexive".
p. 86, l. 26: read "$\triangle$ (a, b, c)" for "$\triangle$ (a, b, 1)".
p. 87, l. 10: after "only if" read "$c > b$".
p. 106, l. 23: read "$a_{n-1} < b_{n-1}$" for "$a_{n-1} > b_{n-1}$".
p. 143, l. 14: read "sequences" for "sequence".
p. 204, l. 22: read "it" for "is".

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Experiments in Living. A study of the nature and foundation of ethics or morals in the light of recent work in Social Anthropology. By A. MACBEATH. London: Macmillan & Co. Ltd., 1952. Pp. xii + 462. 30s.

THIS stimulating book, which is full of matter, contains the Gifford Lectures for 1948-49 delivered in the University of St. Andrews. It is admirably planned and well written. It breaks much fresh ground, and exemplifies a method and indicates a direction of inquiry which should give a new impetus and a new fruitfulness to ethical thinking.

Its purpose is indicated in the sub-title, *viz.* to study ethics in the light of social anthropology. What recent work stresses is the importance of the study of social institutions for the understanding of human behaviour. The institutions of a particular community enable its members on the whole to meet their needs and satisfy their desires, while transforming these needs in the process. Taken together the institutions form an inter-related whole, providing a characteristic way of life, different from the way of life of a community with different institutions. The judgments of any people about what is right and what is good are relative to their whole way of life, and cannot be understood without reference to it.

Professor Macbeath expands and illustrates all this in his five central chapters, describing in some detail the way of life of the Trobriand Islanders, the Tonga of S.E. Africa, the Australian

Aborigines and the Crow Indians (103-244). He devotes two chapters (245-290) to showing that the mental processes and the mental powers of primitives are not different from our own, and argues that it follows that their moral experiences are not different from our own, and that hence our ethical theories must submit to the test of confirmation with their moral life. Two controversial chapters mainly dealing with Malinowski (291-350) are designed to rule out the view that "among primitives, religion is the source of moral authority, the ground of the rightness of moral rules" (322), thus vindicating the autonomy of morals. So far as primitives are concerned, "the content of their moral codes and the principles governing their social institutions were not derived from, and are not dependent on, their religion" (350).

These four chapters leave the way open for his main task. He does not regard the anthropological evidence as relevant to those modern theories which reject the view that moral utterances are assertions of some sort capable of being shown to be true or false, and hence he does not discuss these modern theories at all. He notes merely that, in his view, while they give correctly a partial account of all moral judgments, they are wrong in denying "the assertive element in moral judgments" (vii). What he does hold is that the anthropological evidence definitely rules out any ethical theory to the effect that certain rules of conduct, taken in isolation, can be intuited to be right, or that certain states of affairs, taken in isolation, can be intuited to be good; and he devotes two chapters (351-427) to a consideration of these two views, showing how they break down when confronted with the moral attitudes and assertions of primitive peoples. So far as primitive peoples are concerned, when an individual regards something as a duty, it is "because he sees that [it is] required for the smooth functioning of a form of life which he believes to be on the whole good both for himself and for his people" (183: *cf.* 154-155, 240, 320; on p. 220 the same principle is expressed negatively). And he believes it to be good because of the way it enables him to realise his needs and desires through social activities in which other people realise theirs. What is wrong with theories such as those of Moore and Ross is just their method of isolation of acts and of situations both from one another and from the whole complex of social activities which make up a way of life. It leaves them with the problem, insoluble on their method, of adjusting to one another the various isolated intuitions which bear on any concrete situation.

Professor Macbeath's general reading of the anthropological evidence is as follows. All men start off with the same set of needs, desires and interests, and every society is seeking the same goal, *viz.* the satisfying of these needs, etc., for everyone in a way which is adequate, consistent and comprehensive (90, 65). He rejects the view of Ruth Benedict that different societies are seeking different ends, each making its own selection from the totality of human

interests, though he is willing to agree that human nature and its needs may be satisfied in more than one form of life. Thus the common goal is described only in terms of its criteria. He speaks of it as the formal ideal. Societies follow different paths toward this same goal, through different social institutions, which give distinctive forms to needs, desires and interests in the process of satisfying them. But no particular set of institutions, as actually developed by a community at any given time, completely solves the problem, and people come to behave, with general social approval, in ways which conflict with the strict ideal of behaviour prescribed by this or that institution. The general set of feelings, attitudes, considerations, which cause them to sanction breaches of the strict letter of the institutional law, Professor Macbeath speaks of as their operative ideal. It is that in the light of which their existing institutions, taken strictly, are judged, partially approved and found partially wanting. Their actual behaviour is closer to the requirements of their operative ideal than to those of their institutions taken strictly. The operative ideal can be described as the formal ideal as shaped by a people living under particular institutions. As the name implies, it is operative ideals rather than the formal ideal which guide and sustain the moral activities of the members of a community. But the operative ideal is always incomplete, and it is in the light of the formal ideal that it is judged to be so.

Thus between the raw human nature and the formal ideal, which are the same for all communities, come social institutions and operative ideals, which differ in different communities.

A further important point must be noted. What has been said so far concerns outward activities. From the moral point of view, however, Professor Macbeath holds, what is essential is the spirit in which these activities are carried on. Moral goodness consists in loyalty to the ideal, and in doing what is right because it is right. This is a matter on which he complains anthropologists say very little, stressing rather the incentives to right behaviour set up by social forces. Their evidence does, however, show that many individuals among primitive peoples do think their way of life is good, and do genuinely desire to promote it by their action. Thus moral goodness, loyalty to the ideal, is capable of being achieved just as well by primitive peoples as by people in more complex societies.

His analysis of the anthropological material leads him to a moral theory which he describes as "a reformulation or modification of the self-realisation type of theory" (412). The whole process by which communities are seeking a satisfactory institutional embodiment of the formal ideal is a moral process. No distinction we make between an end, and means towards an end, can be finally adequate, for the institutional activities through which the formal ideal is approached are themselves a part of the embodied end, and

not merely a means to it; and moral goodness, loyalty to the end, is shown in the spirit in which these activities are carried on. But remembering this, we can use the distinction, and say that the ordinary moral rules operative in a given community are on the whole "empirical generalisations . . . as to the conditions necessary for realising the form of life which is worth while"; while "this form of life—whether we call it the moral ideal, the good for man or the good of the self as a whole—consists of desired ends and acts, in and through which they are realised, organised into a pattern with a unity corresponding to the unity of the self as self-conscious" (50). The ends prescribed by needs and interests are themselves non-moral (59); it is only as elements in a way of life judged good that they become moral. They have to be integrated in such a way as to satisfy the whole nature of man; and for this some have to be "subordinated to others, and some have to be realised in certain ways and at certain times, and some repressed altogether" (60).

Let us pause here for a moment and ask, in what way is this a moral process? The answer I think turns on the question what kind of satisfaction we are talking about. Is it merely a general feeling of well-being, a sense that there is nothing to complain about, nothing one would wish altered, a recognition that one's desires are satisfied, or does it contain as a distinctive element a judgment that the life is good? And is it this which makes the process moral, *viz.* that we have here a distinctive kind of judgment, *sui generis*, which is not tested by any feeling of well-being, but is itself the main ground of this feeling?

This would seem to be his view. The moral process of integration requires the subordination of some needs and interests to others. Such subordination can and does often occur without anything moral entering in. But fundamental among the criteria in the light of which this subordination as a moral process has to take place comes the principle of justice, which he describes as a rational principle which "admits of no exception which is not morally wrong" (52). What it requires, he says, is that "where people are treated differently there should be a relevant reason for the difference"—though, as he admits, it is not always clear what reasons are relevant. Its real basis is "the nature of personality and the recognition by the individual that others are personalities, manifestations of the moral consciousness, like himself" (53); and in his account of moral progress this recognition plays the central part (451 f.).

We must ask, on what is the acceptance of this principle based? Why distinguish it from the ordinary moral rules, which as we have seen, are subject to test?

The answer ought to be somewhat as follows. Suppose various ways of life which embodied to a high degree the principle of justice proved unsatisfactory, apparently on account of the working of

this principle, Professor Macbeath would have nevertheless not to condemn the principle, but to seek other features in the ways of life which were responsible for the lack of satisfactoriness. This seems to be part of what is meant by calling it not an empirical rule but a principle. He should not allow it to be capable of being refuted by any empirical evidence.

But in his final treatment of this subject (453-454), he does not go so far as this. He still asserts that the final test of the validity of a principle lies in its capacity to be incorporated in an operative ideal: which would put the principle of justice in this respect on a level with the other moral rules. And this is in accord with his general thesis that "the only judgment which is strictly unconditional and self-justifying is that passed on a way of life as a whole" (356). But this general thesis seems inconsistent with his statement about the principle of justice (453). "It seems to me", he says, "an objective or rational principle. The moral equality of men as persons is not something which depends on us, not something which we want or create. It is something which we discover or recognise and have to accept." Here, it seems to me, is a principle asserted to be unconditional and self-justifying, which is not about a way of life as a whole.

We can perhaps reconcile the two views if we recall that according to him justice requires that if different people are to be treated differently there must be some relevant reason for this: and that we do not know clearly what relevance means, except by trying out various alternatives in an operative ideal. But I am rather unhappy about the effect of this on the claim that we recognise moral equality as a fact.

I should differ from Professor Macbeath on many points of detail in his reading of the anthropological material, as well as on his view that that material is not relevant to the modern theories which regard moral utterances as not assertions in his sense. I should disagree with him on many points of moral theory. But my agreements with him are far more fundamental than any differences. His method of approach, and the general form of his inquiry, seem to me of the first importance; and I admire immensely the skill with which he has carried out his task, and the great learning which lies behind it.

L. J. RUSSELL.

Plato's Theory of Ideas. By Sir DAVID ROSS. Oxford University Press, 1951. Pp. 250. 18s.

THIS book seeks to trace the history of Plato's Theory of Ideas both as expressed in his written works and as further developed—according to the reports of Aristotle and others—in late oral

teaching. Rather more than half is devoted to a study of relevant passages from the dialogues, taken in chronological order. In subsequent chapters some general matters are discussed (for example, the early influences on Plato's thought, and the population of the world of Ideas), and the evidence about unwritten doctrines is examined at some length. Sir David Ross writes with an expected lucidity of style and independence of judgment. Though modern literature is taken account of, the primary texts are kept constantly in view, and polemic—if the friendly airing of differences can be so called—always subserves the main purpose of direct exegesis. The footnotes contain references to all passages cited and there are English and Greek indices. There are no important misprints.

A reviewer cannot provide a just summary of a work whose main strength consists in many particular arguments and interpretations; and in this case any such attempt is the more dispensable as the book will certainly, and deservedly, be read by all who interest themselves in Plato. This being so, some observations of a mainly critical nature may be in place.

The philosophical vocabulary Ross uses is not always luminous. Consider, for instance, the question whether Plato held the Forms to be immanent or transcendent. This question is evidently thought to be tolerably clear and somewhat important;[1] but on closer scrutiny it seems to lose most of its clarity and some of its importance. We learn (p. 19) that if Forms have separate existence they are transcendent and that if they are they cannot be perfectly exemplified in particulars. A little later (p. 21) Ross speaks of a passage as affirming 'not the separate existence of the Idea . . ., but its difference from its embodiments, and its eternity and purity in contrast to their transitoriness and imperfection'. Thus it appears that while transcendence entails imperfect exemplification the imperfection of particulars does not entail the transcendence of the relevant Form. Further on, however, stress on the imperfection of particulars is taken as a mark of the transcendental view; the use of the 'participation'-vocabulary is taken to imply the immanence of the Form, the use of 'imitation'-words its transcendence. This procedure is inconsistent with the previous statement that the contrast between perfect Form and imperfect particulars does not involve transcendence; and the divergence makes it difficult to know what the question at issue really is.

Even in his final remarks Ross seems to oscillate. The only conclusion possible, he says (p. 231), seems to be that, while Plato

[1] At one point Ross says that in face of fundamental community of thought between Plato and Aristotle 'the question on which Aristotle lays so much stress, whether universals exist apart from particulars or not, seems almost to be a question of words' (p. 226). If so, all the more reason to be clear about the meaning of the words. But anyway Ross does attend to the question often and at length.

was not quite satisfied with either 'imitation' or 'participation', 'he saw no way of getting nearer to the truth than by using both, the one stressing the intimacy of the link between a universal and its particulars, the other stressing the failure of every particular to be a perfect exemplification of any universal'. This ascribes to Plato a balanced appreciation of two features of universals. But two pages later Ross writes: 'As Plato's mind matured he moved gradually towards a transcendental view of the Ideas as entities existing on their own account and only imperfectly mirrored in sensible things and human actions.' It is surely odd to construe Plato's use of different terminologies *both* as an attempt to convey by metaphor different facts about universals *and* as evidence for a gradual re-allocation of Ideas to a higher realm of being. The latter construction is the more strange since, according to Ross's own analysis, 'participation'-language continues to be used in the latest dialogues. The 'difference of stress' theory might account for this, but it seems fatal to the view that Plato moved gradually towards an ontological re-assignment of the Ideas, and, indeed, to the whole notion that these terminological differences are necessarily reflexions of important differences of doctrine as to the 'ultimate status' of universals.[1] To reinforce this conclusion one may refer to Aristotle. Having asserted that the mature Plato held a transcendental view Ross says: 'And it is naturally of the later Plato, at whose feet he had sat, that Aristotle writes, and against him that he truly formulates the charge that he separated the Ideas from sensible things.' But Aristotle is the worst witness Ross could have called. For it is abundantly clear both that he knew of no distinct earlier form of the Platonic theory in which the Ideas were *not* separate, and that he supposed that Plato used *both* 'imitation' *and* 'participation' with reference to 'separate' Ideas.[2]

It must not be thought that this whole book is centred round the question of transcendence and immanence. Yet I cannot help thinking that the traditional preoccupation with this sort of question and the traditional talk of '*the* theory of Ideas' and of Platonic 'theories' and 'doctrines' have influenced Ross's general approach, making it too like that of Aristotle in his doxographic remarks. We have learned to reconstrue Aristotle's own philosophic life not as one enormous piece of system-building, not even as a single-track, one-way development, but as containing a complex variety of changes and oscillations, of difficulties discussed with candid

[1] I am arguing here only against the internal consistency of Ross's position. What is *really* striking about his analysis is that only the *Timaeus* of the dialogues he regards as late contains '*imitation*'—talk (his reference to *Theaetetus* 176 e 3 contradicts p. 101). *This* fact may suggest important inferences. See G. E. L. Owen's paper in C.Q. 1953.

[2] It is interesting to read Ross's own note on *Metaphysics* 987b10 (vol. i, p. 162 of his edition).

inconclusiveness and bright ideas suggested tentatively. Something of the sort may be said of Plato also. His mind, like his dialogues, was of remarkable flexibility, zealous to spot and pursue new problems, to try out solutions and refinements and modifications to them, to test arguments and to clarify meanings. Philosophy for him was an activity rather than a set of doctrines, an open-eyed journey not a mystical destination. To evoke his spirit must be to re-live the complex movements of his thought and not primarily to extract bits of theory from them. The early Academy formulated a number of 'arguments for the theory of Ideas', and to take note of this plurality is important for understanding Plato. It is imprudent to speak of *the* 'foundation of Plato's metaphysical theory' (pp. 101, 103); or to approve Aristotle's over-simplified account (which is surely only his logical analysis, not first-class biographical evidence) of 'the origination in Plato's mind of the theory of Ideas' (pp. 159-160). Even the many Academic formulations are mere embalmings of Plato's living thought, which found its proper expression in supple dialogues.

Some specific points. (1) Ross makes no mention of *Republic* 523-524, which is necessary for the elucidation of the end of Book V, which throws great light on one motive for positing Ideas (one sort of 'imperfection' the sensible world manifests), and which notices a difference of kind between some Ideas and others. Other passages relevant in this connexion and unnoticed by Ross are *Phaedrus* 250 and 263a, *Cratylus* 432, *Politicus* 285-286, *Alcibiades I* 111. (2) Some later dialogues show less concern with knowledge and its presuppositions than with the necessary conditions of significant talk in general. *Parmenides* 135, *Sophist* 251-259 and *Philebus* 14-18 should be considered together. At the least this is a distinguishable strand in Plato's thought; at the most it represents the abandonment of belief in or concern for the theory of Ideas as metaphysical objects. In this context one would have expected some reference to be made to remarks of Professor Ryle and Mr. Robinson.[1] (3) On pages 172-173 Ross argues that Forms of artefacta are required 'by the general doctrine that wherever there is a common name there is a Form'. On the next page he expounds the Platonic principle that 'not any and every subdivision of a class which ingenuity can devise answers to a subdivision ordained by nature'. This latter principle must surely entail some refinement at least of the general doctrine mentioned. Weighty questions are involved which demand not only consideration of the purpose of Division and Collection but also study of what Plato says in various places about *στοιχεῖα* and *συλλαβάι*, *ὅλα* and *μέρη*. (4) In some late dialogues, notably the *Sophist*, Plato concerns himself with what we might call formal concepts;

[1] G. Ryle, "Plato's 'Parmenides'," MIND, 1939, especially p. 315. R. Robinson, "Forms and Error in Plato's 'Theaetetus'," *The Philosophical Review*, January 1950, especially pp. 18-19.

and one must ask oneself to what extent he was aware of the difference between formal and material or sort-concepts, and what he thought about it if he was aware of it. It is a pity that Ross was apparently unacquainted with Ryle's articles on the *Parmenides*, in which these questions are discussed. But the feeling that something very special is going on in the *Sophist* is an old one,[1] and some examination of these problems ought to find a place even in an avowedly historical work on the theory of Ideas.

I wish to make some observations on two special topics; and first τὰ μαθηματικά. Ross's views on the nature and motivation of this doctrine are in the main already familiar from his edition of Aristotle's *Metaphysics*. In the closer treatment called for in this book there seems to be a lack of consistency. And if this is so it is worth pointing out: for the difficulties encountered by those who do ascribe a doctrine of Intermediates to Plato constitute one of the main arguments of those who do not. Ross says that the phrase αὐτὰ τὰ ἴσα of *Phaedo* 74c1 is 'an allusion to mathematical entities which are neither Ideas nor sensible things, an allusion which paves the way for the doctrine of the "Intermediates". Plato very likely at this stage did not himself appreciate the significance of his own allusion' (p. 25). Later, however, he says that 'the passage does not necessarily imply a belief in the existence of perfect equals' (p. 60). He holds that the Line in the *Republic* does not involve Mathematicals, and writes: 'The discovery of the μαθηματικά was a great innovation. . . . Now anyone who is familiar with Plato's writings knows that he is nothing if not explicit' (p. 59). And in view of the Seventh Letter, which conspicuously does *not* mention Mathematicals, Ross concludes (p. 62) that it was not till after that Letter (*i.e.* in the last five years of his life) that Plato formulated the doctrine, though he had long been on the point of formulating it. The distinction between Forms and Mathematicals is 'below the surface in dialogue after dialogue, only waiting to be made explicit' (p. 66). But if Plato is nothing if not explicit, and if the discovery of Mathematicals was a great innovation, what is meant by saying that he is in the dialogues constantly on the verge of stating the doctrine but never quite does so? Is it that he has *not* (yet) discovered these important entities though he ought to have noticed them by now; or is it that he keeps them up his sleeve? One thing, anyhow, seems settled, that the first formulation of the doctrine was after the Seventh Letter; and the statement (p. 86) that it provided in one department the answer to a difficulty mooted in the *Parmenides* is consistent with that chronology. When Ross later asks what Plato thought about the Mathematicals in his last period he conjectures (p. 223) that he

[1] Michelis, for example, writing in 1859, came near to attributing to the *Sophist* 'eine klare Erfassung des Unterschiedes des Formal- und Real-begriffes, die Unterscheidung des Formalen und Realen im Denken', *Die Philosophie Platons*, p. 201.

came to think of them as mathematical fictions. This gives a possible though rather cramped chronology : in the last five years of his life Plato both formulated the theory of Mathematicals for the first time and came to re-interpret them as not entities but fictions. The next page, however, throws all this into the direst confusion. After quoting the *Timaeus*, which he dates before the Seventh Letter, Ross writes : 'Thus the intermediates form no part of Plato's cosmological scheme, and it seems highly probable that he had come to regard them as fictions necessary for geometry but having no place in reality'. So before his first statement of this great innovation Plato had left it behind. Ross continues : 'It is possible that even when he wrote the *Republic* he had come to think this, and that this is part of his meaning when he speaks of dialectic as annulling the hypotheses of mathematics'. Besides contradicting Ross's earlier interpretation of the Line and of the phrase *ἀναιροῦσα τὰς ὑποθέσεις* this sentence seems to destroy any coherence which previous remarks about Mathematicals may have had.

Finally, some comments on Ross's use of Aristotelian evidence in his discussion of the alleged *ἄγραφα δόγματα* of Plato. (*a*) Ross seeks first to establish against Professor Cherniss the existence of such late oral teachings and the possibility of reconstructing them from the reports of Aristotle and others. But he does not do justice to Cherniss's thesis and arguments (as outlined in *The Riddle of the Early Academy*). For Cherniss does not assert that no oral instruction was given in the Academy but that it was restricted to mathematics (*Riddle*, p. 82). And, more important, he does not deny that Plato delivered a lecture *περὶ τἀγαθοῦ* and that Aristotle and others heard it and wrote books about it : discussion of this lecture occupies a large part of chapter i of *The Riddle*. Ross says (p. 147) that the single reference to *ἄγραφα δόγματα* in *Physics* 209b14 'is in itself enough to refute the contention that all that Aristotle says about Plato is derived from the dialogues'. But Cherniss's view is not so crude that it is refuted by the mere mention of that reference, which is, in *The Riddle* (pp. 15 ff.), recognised and examined. The crucial question is what inference can be drawn from the evidence. Cherniss's arguments about this—which involve an examination of the sources, motives and coherence of Aristotle's statements—receive no adequate attention in Ross's book. (*b*) In the absence of effective general refutation of Cherniss's position particular arguments lose much of their force. For instance, Ross points out that Aristotle never cites any dialogue as evidence of any of the later developments he ascribes to Plato and infers that he was drawing upon another source, the *ἄγραφα δόγματα* (p. 149). Against the simple thesis that all Aristotle says comes from the dialogues the lack of citations might be a strong argument. But one who holds that Aristotle was wont to draw out what he thought people ought to have said or must have

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meant, and to reduce all views to the categories he thought appropriate, would find no difficulty in the absence of chapter-and-verse citations. He would point out further that the one explicit reference to ἄγραφα δόγματα is at the same time a direct citation of the *Timaeus*, a citation moreover which, as Ross agrees (p. 221) shows Aristotle making two obvious blunders about this dialogue. Ross says: 'Since Aristotle certainly makes these mistakes, it is easy to suppose he made yet another' (p. 222); and a plain statement of the *Metaphysics*[1] is 'corrected' so as to make the alleged Platonic theory more sensible. It would surely be more methodical to dally over that remarkable reference to the *Timaeus* and to see what it implies about Aristotle's methods and understanding of cardinal points of Plato's teaching. Cherniss's arguments about this (*The Riddle*, pp. 15 ff.) are not considered by Ross. (*c*) Ross asks (p. 150) where in the dialogues we shall find any basis for Aristotle's statement (*Physics* 206b32) that Plato carried the derivation of the numbers only as far as ten; and concludes that this came from oral teaching. Later (p. 179) he says that we must not take that statement too literally since Plato must have believed in eleven-ness as much as in ten-ness. But he does not point out that in a passage of the *Metaphysics* (1084a12-17) which he himself says (p. 152) is concerned with Plato, Aristotle raises the objection that ten numbers will not be enough to take care of all the Ideas there are. So, either he was quite wrong and did mean his statement literally, or he was capable of making for his own purposes a thoroughly illegitimate criticism. Either of these assumptions should have profound bearings on our handling of all his evidence and on our conception of what went on in the Academy. (*d*) Ross writes: 'There is an imposing array of passages in which Aristotle says, or clearly implies, that for Plato all the Ideas were numbers' (p. 216). One of these, however, 'suggests that Aristotle's statement that Plato identified the Ideas with numbers was based on his inference from what Plato said, rather than on a plain statement by Plato'. To suggest that this oft-repeated statement is a (mistaken) inference of Aristotle is surely to suggest something with very grave implications. Ross's subsequent decision (p. 218) that Aristotle's statement must not be taken 'too literally' does not help matters much, even if it is meant to imply—what is unplausible—that Aristotle did not himself take it literally. For he is ready to argue against it in its literal significance; and if he could do this in the full knowledge that it was not held literally what hope have we of recovering any real oral teachings of Plato from Aristotle's polemics? These remarks are not intended to settle any question, but to re-open questions which Ross has bypassed rather than answered.

[1] *Metaphysics* 988a8-14. See *The Riddle*, pp. 20-22.

It would be quite wrong to end on a critical note. This book contains much learning and wise judgment and will be a valuable standard work for many years. It is important in such a case for a reviewer to expose the weaknesses which he thinks he sees, but also to make clear that these are defects in a good book and not representative of its quality as a whole.

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VII.—NEW BOOKS

Einführung in die Logistik, vorzüglich in den Modalkalkül. By OSKAR BECKER. Westkulturverlag Anton Hain. Meisenheim/Glan, 1951. Pp. 92.

Untersuchungen über den Modalkalkül. By OSKAR BECKER. Westkulturverlag Anton Hain. Meisenheim/Glan, 1952. Pp. 87.

BECKER'S *Einführung*, it seems, in the first place addresses the student of philosophy or mathematics who has no first-hand knowledge of symbolic logic. It is headed by a fairly comprehensive account of Boolean algebra. Then follows a short chapter on the propositional calculus. The third chapter deals with the lower functional calculus; a fair amount of space is devoted to monadic predicates; relations are treated but briefly. In this connexion the reader is also introduced to the *Sequenzen-Kalkül* of Gentzen and Hertz. The concluding fourth chapter is reserved for modal logic.

I am not convinced that the *Einführung* is altogether well suited as an introduction. The uninitiated reader will hardly get to understand how certain important logistic techniques which are mentioned (*e.g.* transformations into so-called normal forms or formal deductions from axioms) are used in practice, for purposes of proof. A fuller and more accurate description of these techniques would have been possible within the space at disposal by omitting less relevant material. The meta-theoretical problems of logical calculi, such as independence of axioms, completeness, consistency, and decision methods, receive very little attention.

Original, and important, contributions to the subject are found in the last section of the *Einführung* and in the *Untersuchungen über den Modalkalkül*.

Becker develops a system of modal logic of his own. I shall here call it ML. It can be described in outlines as follows:

The system consists of two parts or stages. The first stage is the ("classical") logic of propositions (PL). At the second stage are added one primitive idea, two axioms, and one rule of inference. The new primitive idea is the concept of necessity (N). As is well known, the modal notions of possibility, impossibility, contingency, strict implication, etc. can be defined in terms of necessity. The first axiom says that two propositions are conjunctively necessary, if and only if they are separately necessary. In symbols: $N(p \,.\, q) \equiv Np \,.\, Nq$. The second axiom says that a necessary proposition is true. In symbols: $Np \supset p$. The new rule of inference (substitution) runs: If the equivalence of two expressions, not containing N, can be proved in PL, then the two expressions are intersubstitutable in any formula of ML.

By a modal expression of the first order or of the first degree I shall understand a formula of ML, in which no two Ns "overlap". The fragment of ML which considers only first order expressions I shall call ML_1.

It is not difficult to prove that Becker's system is a weakened form of Lewis's S2 or System of Strict Implication. (Every theorem of ML is thus also a theorem of S2, but some theorems of S2 are not theorems of ML.)

The author shows that the axioms of ML are independent. A number of theorems are proved. The theorems are, on the whole, known from

earlier treatments of modal logic, but the proofs are often much simplified. The author also discusses the problems of effecting an "order of strength" between modal expressions of different degrees by means of additional axioms. These axioms lead to systems of modal logic such as S3, S4, and S5, or to related systems.

The conception of modal logic as a super-structure based on propositional logic appears to me a happy idea. It simplifies the formalism considerably as compared with the "classical" procedure of C. I. Lewis. An additional advantage of the "two-stage-method" is that it emphasizes the analogy in formal structure between modal logic and the logic of the quantifiers (the functional or predicate calculus).

The analogy between modalities and quantifiers is used in the *Untersuchungen* for an interpretation of ML. Becker calls the interpretation "statistical". The term does not seem to me well chosen. Also the account which Becker gives of the interpretation is not as detailed and rigorous as would have been desirable for a final assessment (*cf. infra*). That the interpretation is both possible and interesting is, however, beyond doubt. I shall here try to give a somewhat incomplete but, I hope, faithful account of its basic features:

Consider first modal expressions of the first degree only. The formulae of PL are interpreted as propositional functions of a variable x. The statement that a formula of PL stands for a necessary proposition is interpreted as meaning that the corresponding propositional function is true for all values of x. For example: If Px is the interpretation of p, then (x)P(x) is the interpretation of Np. It follows that the statement that a formula of PL stands for a possible proposition means that the corresponding propositional function is true for some value of x.

On this interpretation, the theorems of ML_1 become (valid) theorems of the lower functional calculus for monadic predicates. This fact, one might say, constitutes a "verification" of ML_1.

Consider next higher order modal expressions. There are various ways in which they can be interpreted using quantifiers. The author favours the following course:

We divide the Universe of Discourse (the range of all values of x) into classes. The statement that something is necessarily necessary now means that in *all* classes of x's *all* the x's satisfy a certain propositional function. That something is possibly necessary means that in *some* class of x's *all* the x's satisfy a certain propositional function. That something is necessarily possible means that in *all* classes of x's *some* of the x's satisfy a certain propositional function. That something is possibly possible, finally, means that in *some* class of x's *some* of the x's satisfy a certain propositional function.

In order to cope with modalities of the third degree we have to introduce classes of classes of things, and so on.

The interpretation of modal expressions containing parts of different degrees calls for special attention. The author is not very clear on the subject. If a formula of ML contains, say, both NNp and Np, Becker takes the N in Np to refer, not to the Universe of Discourse as a whole, but to some special class of objects within the universe (*Untersuchungen*, p. 23). This is done in order to secure that NNp be a stronger assertion than Np (*ibid.*, p. 20). There is nevertheless something arbitrary about the adopted procedure and one would wish the point further elaborated.

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On Becker's interpretation the theorems of ML become (valid) theorems of a fragment of the higher functional calculus. The statistical interpretation is thus used to "verify" ML.

It is easy to see that the statistical interpretation does *not* "verify" some alternative modal logics. Consider, *e.g.* the distinguishing axiom of Lewis's S4. It says that a necessary proposition is necessarily necessary. When interpreted "statistically" (in Becker's way), this means that if all the x's in a certain class of x's satisfy a certain propositional function, then all the x's in all classes of x's satisfy the same propositional function. This, manifestly, is no logical truth. Hence the axiom is "statistically invalid". The same is readily shown for suitably chosen propositions of S2, S3, and S5.

The existence of several, independent and consistent, systems of modal logic has worried some philosophers of logic. Becker claims [1] for his statistical interpretation the status of an objective and universal criterion, whereby to decide which one of the possible formalisms reflects the "natural" logic of modal concepts. Such a claim, however, for several reasons cannot be accepted:

1. The statistical interpretation, as given by Becker, can easily be modified so as to fit, not ML, but some other modal logic.
2. There are other interpretations of modal logic, beside the statistical one, for which the same claim could be raised with an equal right.
3. The statistical interpretation conflicts with certain highly plausible and common-sensical intuitions about the modal concepts. I shall mention one example only. It is plausible to think that any proposition which follows from a necessary proposition must itself be necessary. It is also plausible to conclude from the truth of a proposition to its possibility. Hence, if a proposition happens to be necessarily true, it will also be necessarily possible. In symbols: $Np \supset N \sim N \sim p$. This theorem is easily proved in S2 (and in any of the stronger systems). But it is not provable in ML and is, indeed, "statistically invalid". For, interpreted "statistically", the theorem runs: If, in a certain class of x's, all the x's satisfy a certain propositional function, then in all classes of x's some of the x's will satisfy the same propositional function. This is no truth of logic.

It seems to me, therefore, that if any one formal system has to be singled out as being the "natural" logic of modal concepts, then this system must be stronger than Becker's system.

The very problem, however, of *deciding* between various modal logics does not appear to me important. The urgent problem in this connexion is rather that of *interpreting* the systems, thus showing under what conditions and for what purposes they have a meaning and a use. Particularly promising appear to me such interpretations of modal logics which relate the notions of possibility and necessity to various types of effects (dispositions, potentialities) among natural phenomena.

In the sequel Becker gives a second interpretation of ML. He calls it the normative interpretation. It can be described as follows:

Let p mean (the doing of) a certain action and $\sim$ p the neglecting (not doing) of the same action. Np is interpreted: the action p is obligatory.

[1] *Cf. Einführung*, p. 5 and *Untersuchungen*, p. 22; the scope of the author's claim, however, is not quite clear to me.

$N \sim p$ accordingly means: not doing p is obligatory, or: p is forbidden. $\sim N \sim p$ means: p is not forbidden, or: p is permitted.

The order of modalities can be made to answer to an order (hierarchy) of command-issuing authorities. Thus NNp is interpreted: the higher authority commands the lower authority to command p. $N \sim N \sim p$ accordingly means: the higher authority commands the lower one to permit p. $\sim N \sim Np$ means: the higher authority permits the lower one to command p. And $\sim NN \sim p$ means: the higher authority permits the lower one to permit p.

This extension of the normative interpretion of modal concepts to higher order modalities is an interesting novelty.

In order to show, however, that the interpretation really fits ML it is not enough to interpret the concepts of the calculus only. We have also to interpret the axioms (and rules of inference). On this point Becker is not completely successful.

The distribution axiom presents no difficulties. The formula $N(p \,.\, q) \equiv Np \,.\, Nq$ means in the normative interpretation: It is obligatory to do both p *and* q, if and only if it is obligatory to do p *and* obligatory to do q. And this, plainly, is true about obligation.

The second axiom, however, causes some trouble. The formula $Np \supset p$ consists of two parts. The first part, Np, stands for a proposition. The second part, p, according to Becker's explicit statement (*Untersuchungen*, p. 41) does not stand for a proposition but for an action. In discussing the axiom he nevertheless (tacitly) assumes that it can be interpreted as a proposition.

The "propositional interpretation" of p cannot be: the action p is done. For then $Np \supset p$ would read: if an action is obligatory, then it is done. And this, alas, is not always true.

In order to cope with the difficulty, Becker modifies the notion of an "action" so as to mean "legitimate action" (*legale Handlung*). According to Becker, $Np \supset p$ now reads: if p is obligatory, then p is in fact legitimately done. (*Wenn p geboten ist, findet es tatsächlich legal statt.*) This, Becker says, is true. For, if p were not actually done, it would have been illegitimately neglected, which contradicts the assumption that we are only dealing with legitimate actions (and negation-actions).

The whole argument is obscure and, as far as I can see, untenable. It is obvious that the notion of "legitimate action" itself involves a normative element. It is plausible to regard it as equivalent to "permitted action" (*i.e.* permitted by the legal code or authority under consideration). Let us assume that "legitimate action" can be thus understood. The only correct way to a "propositional interpretation" of (the second) p which I can see would be to equate p with $\sim N \sim p$. The axiom $Np \supset p$ would then be but a disguised form of $Np \supset \sim N \sim p$, which says that if an action is obligatory, then it is permitted. This can truly be regarded as stating a logical truth about the notions of obligation and permission.

In whatever way the muddle is cleared up, it seems to me quite certain that the normative interpretation is not valid for ML. There is indeed a normative interpretation of modal logic, and maybe even of several systems of modal logic, but this modal logic or these modal logics are not identical with Becker's system. They differ from ML in that they do not contain the axiom $Np \supset p$ (or an equivalent to it). Instead, they contain an axiom $Np \supset \sim N \sim p$ (or some axiom from which this

formula can be proved). They may also differ from ML in some other features which need not, however, be discussed here.

I have tried to criticize some vital points in Becker's treatment of modality. But I hope that I have also succeeded in showing that Becker's *Untersuchungen* is an interesting book. It deserves reading by all students of modal logic, and also by those interested in the broader philosophic aspects of the subject. It might be added that there is a separate chapter in the *Untersuchungen* devoted to an examination of some philosophical doctrines of modality (in particular the one of Nicolai Hartmann) in the light of modern (symbolic) modal logic.

G. H. VON WRIGHT

Deutung und Darstellung der theoretischen Philosophie Kants. By FELIX GRAYEFF. Richard Meiner Verlag, Hamburg. Pp. xxiii + 226.

DR. GRAYEFF, as a German who was a lecturer in a New Zealand University and has been influenced by English interpretations of Kant, has set himself a double aim—firstly, to explain sentence by sentence the most fundamental passages in the *Critique of Pure Reason*; and, secondly, to set forth a new principle of interpretation. To this task he has obviously devoted an immense amount of labour, and the resu't is a book packed with thought—at times so tightly packed that it offers difficulties to the reader.

In his detailed commentary his aim is to establish the unity of Kant's thought. He is bold enough to claim that in the *Critique* every proposition is consistent with itself and with every other proposition. Not content merely to explain separate sentences in Kant's own terminology —a humble but necessary task—he seeks to interpret them in a terminology of his own. This provokes, and demands, a considerable effort of thought; and although his interpretations contain much that is valuable their meaning is not always wholly clear.

The adoption of a novel terminology is presumably made necessary by Dr. Grayeff's views about the principle and method of interpretation. On this topic it is hardly possible to describe his theory briefly, and he indicates himself that it is only partly expounded within the framework of his short book. Interpretation for him—to speak very broadly—is concerned with the logical structure and logical unity of the *Critique*, and its aim is even said to be 'the concept of logic itself, that is, the concept of coherent thinking as such'. The logic in question is, however, neither traditional logic nor the logic expounded by Kant, but a logic which has itself to be derived from a study of the *Critique*. This new logic appears to question the necessity of the Principle of Contradiction and so to transform Kant's conception of the difference between analytic and synthetic propositions. Hence, even if Dr. Grayeff holds that he is only making explicit what is implicit in the *Critique*, it is not always easy to be sure whether he is interpreting Kant or improving upon him.

According to Dr.Grayeff, Kant sees the traditional formal logic as a logic of clarificatory thinking, and his own transcendental logic as a logic of constructive thinking—that is, a logic of the thinking by which phenomenal reality is constructed. This is interesting so long as it does not obscure the fact that for Kant formal logic is a logic of thinking *as such*. There is a danger of supposing that formal logic is the logic of a

XUM

special kind of thinking—namely, analytic thinking (or analytic propositions). At times Dr. Grayeff seems to go even further and *identify* formal logic with analytic thinking, although one ought always to distinguish the thinking *in* logic from the thinking logic thinks *about*. He also says that mathematical conclusions are analytic—apparently because they follow from premises in accordance with the Principle of Contradiction. If he is using Kant's terminology, this is an error, as is clear from B 14: a conclusion is analytic for Kant if the premises are analytic, and synthetic if the premises are synthetic. Slips of this kind are apt to disturb confidence, and it is hard to be sure that Dr. Grayeff has wholly freed himself from the traditional misunderstandings disposed of by Professor Klaus Reich in his *Vollständigkeit der kantischen Urteilstafel*, a work of immense value even to those who are not prepared to accept its conclusions.

It is obvious that transcendental logic cannot justifiably be based on formal logic, if formal logic deals with a special kind of thinking and transcendental logic with another special kind of thinking. It should also be obvious (although many have been blind to it) that the forms of judgment as given in formal logic must be the forms of judgment *as such* and not merely the forms of analytic judgments: indeed the forms of judgment themselves cannot conceivably be *either* analytic *or* synthetic since this distinction is one of matter and not of form. Unless this is accepted, Kant's attempt to connect the categories with the forms of judgment is not merely mistaken but absurd. The relation between the two kinds of logic is fundamental for any understanding of the *Critique*. Dr. Grayeff is fully aware of this, and he knows well that for Kant transcendental logic is based on formal logic. In spite of this I fail —perhaps through my own fault—to get from him a clear account of the relation between the two logics; and he tends sometimes to speak of formal logic as if for Kant it merely supplemented or completed transcendental logic. Even as regards the crucial paragraph (B 104-5) of the Metaphysical Deduction—which, heaven knows, is difficult to understand —he implies that the forms of judgment and the categories are produced by two different, but *corresponding*, acts of the understanding, while Kant himself is emphatic that they are produced by precisely the same acts.

Dr. Grayeff's book is too difficult for beginners. Those who are more expert will find, if they take the necessary trouble, that it is always suggestive, even if it is hard to grasp as a whole and sometimes—perhaps because of this—not altogether convincing in its details.

H. J. PATON.

Leslie Stephen—His Thought and Character in Relation to its Time. By NOEL ANNAN. McGibbon and Kee. Pp. 342. 25s.

LESLIE STEPHEN is not an attractive figure: hearty and even blustering, the partisan of heartiness and masculinity, preoccupied with "effeminacy" and morbidness. Nor is he of any importance as a philosopher. Mr. Annan is forthright about this. "Stephen's book (*The Science of Ethics*) is, therefore, worthless *as ethics*; it is simply an expression of opinion about a scientific process related to ethics by crude biological analogies" (p. 271). In this interesting biographical study Mr. Annan makes the best of him by setting him very firmly in context, carefully tracing his

genealogical and personal connexions with that Victorian aristocracy of intellect descended from the Clapham Sect, using the stages of Stephen's spiritual biography—his evangelical origins, a rationalism derived from Mill's *Logic*, his loss of faith and defence of agnosticism, his venture into evolutionary ethics, the moral preoccupations of his biographical and critical writing—as starting points for illuminating excursions into the aspects of Victorian intellectual life of which they form part. Even the more personal details of biography are treated as symptoms rather than for their own sake. *E.g.* his mountaineering; "Stephen went to the Alps to climb and for no other reason" (p. 81). But, "like love, mountaineering makes it possible for the intellectual to experience things which would otherwise be impossible: danger, intense comradeship, manliness, physical pain in pursuit of a tangible objective, and the sensation of being at one with nature" (p. 82). At times this pursuit of wider affinities in his subject-matter leads Mr. Annan too far. On pages 250-255 the whole field of English literary criticism is presented in terms of a rather unfortunate debate between aesthetic and moralising critics.

Chapters 6, 7, and 8, describing his opposition to orthodox religion, his ethical theory, and the moral principles underlying his biographical and critical writing, will be of most professional interest to readers of MIND. Religion, for Stephen, was primarily a set of beliefs and he was just bluntly unable to see that Christian beliefs were in the least warranted. He attacked Broad Churchmen as unwitting self-deceivers, theologians like Maurice as sophisticated ones. The former could not honestly square their reinterpretations of doctrine with the Thirty-Nine Articles, the latter were debasing the currency of language. Attempts like Arnold's to provide some diluted religious substitute to satisfy emotional needs, he stigmatised as irrational. Yet his own evolutionary theory of ethics, which saw moral principles as the "conditions of social welfare", was just such an *Ersatz*. "Evolution replaces God", says Mr. Annan, "The power of religion over the very minds which denied it is nowhere more subtly instanced than in Stephen's evolutionary ethics" (pp. 291, 221). A true English Pelagian, Stephen saw importance for religion only in providing beliefs that would certify principles of conduct. His open-air temperament involved a prosy ignorance of the abyss. "Myth"—any type of non-rational belief—could only, in his view, discredit morality.

Rightly distinguishing the reasons for moral convictions from their causes, Stephen surprisingly held the latter alone to be worthy of philosophical study. But this, as Mr. Annan points out, makes things harder, since it is easier to answer questions of morality than questions of social welfare. The supposedly scientific basis of his new sanction for virtue was in no better case than its claim to be philosophical. His "social science" was no more than *a priori* anthropology and, on the other hand, conscience and the moral reformer are hard to reconcile with "the public spirit of the race".

It is somehow pathetic to think that it was on this that Mr. Ramsay was presumably working in *To The Lighthouse*.

ANTHONY QUINTON.

An Introduction to Philosophy of History. By W. H. WALSH. Hutchinson's University Library, London, 1951. Pp. 173. 7s. 6d.

THE basis of this lucidly written and scholarly introduction to the philosophy of history is a distinction between critical philosophy of history, *i.e.* the epistemology of historical enquiry, on the one hand, and speculative philosophy of history, *i.e.* the extraction of some pattern or meaning from the manifold of historical events, on the other. Mr. Walsh considers that the main questions asked in critical philosophy of history are: (1) In what ways is historical knowledge like and unlike the knowledge characteristic of the natural sciences? (2) What is the nature of historical explanation? (3) What sort of thing are historical facts? (4) Is it reasonable to expect impartial enquiry to lead to assured and agreed accounts of the human past? As to (1), Mr. Walsh stresses the concern of history with particular human actions. He notes the logical similarity between the natural scientist's prediction of future events and the historian's "retrodiction", on the basis of present evidence, of the deeds that must have been enacted. (Incidentally, he attributes the word "retrodiction" to Professor Ryle. It is used, however, in the sense under discussion, by J. M. Robertson in *Buckle and his Critics* (1895).) Retrodiction, Mr. Walsh admits, appears to assume a knowledge of laws of human nature, just as prediction in the natural sciences is made possible by a knowledge of physical laws. But on his view what makes historical retrodiction vastly different from scientific prediction is that the historian's knowledge of human nature may (must?) have features that link it with creative literature as well as with common sense and scientific psychology. As to (2), Mr. Walsh borrows Whewell's word "colligation" and uses it for the ordering of historical events under dominating concepts and in the light of human purposes, suggesting that historical explanation is the establishing of this sort of teleological or quasi-teleological intelligibility. In arguing for this he discusses the views of Dilthey and Collingwood, but surely Max Weber's highly relevant conception of an "ideal type" should not have been ignored in such a context. In considering (3), Mr Walsh shows the difficulties in the way of regarding historical truth as either coherence or correspondence with facts, and suggests a compromise view. But compromise is of no avail, for the siren song of metaphysics has already been heard. And so, under (4), Mr Walsh tries to mitigate the historical scepticism that arises from considering the different moral and metaphysical presuppositions with which historians approach their subject. He suggests a "perspective theory", according to which historians are regarded as explaining historical events from different points of view, such as that of liberalism, or Marxism, or Protestantism. Such explanations, he argues, though not universally acceptable, could nevertheless be compared in respect of their honesty in presentation and adherence to the rules of technical historical competence. At this point, it is clear, the distinction between critical and speculative philosophy of history has broken down, as the author himself feared it might.

In his discussion of speculative philosophy of history, Mr. Walsh introduces and criticises the views of Kant, Herder, Hegel, Comte, Marx, and Toynbee. In doing so he presents an admirable set of rapid characterisations, but there are three points of detail I should like to touch on. (*a*) He regards Comte as a speculative philosopher of history because he supposes him to have forced the facts into the "rigid framework" of his

Law of the Three Stages. He does not observe, however, that this Law is based, in the first instance, on the history of the natural sciences, for which it has considerable plausibility, and was applied to universal history because of the importance attached by Comte to intellectual development. If Comte is to be regarded as a speculative philosopher of history, then it should be emphasised that his speculations are based on his views about the nature of science, *i.e.* the speculative element in his view was mixed up with the critical elements. (*b*) Marx, according to Mr. Walsh, has provided historians with "a sort of recipe for producing empirical hypotheses". The recipe, if I follow him, is to mix classes dialectically into an "economic background". It would have helped if a single example of a successful historical hypothesis formed in this way had been given, but I am afraid that this would be very difficult because the instructions comprised in the recipe are so extraordinarily obscure. I suggest that such notions as "economic background" are among the objects that philosophers of history should give close attention to. (*c*) In the course of a very interesting account of Hegel's philosophy of history, Mr. Walsh suggests that Hegel made a substantial contribution to historical studies by being "one of the first to write a history of philosophy". But many had written *on* the history of philosophy before Hegel, and quite a number of comprehensive histories of the subject had been published in the seventeenth and eighteenth centuries. I hardly like to suggest that Mr. Walsh, like Collingwood, has been unduly impressed by the vigour of Hegel's self-praise at the beginning of his lectures on the *History of Philosophy* where he announces his contempt for his predecessors in that field. But it should be mentioned that Tennemann's *History of Philosophy*, the first volume of which was published in 1798, was by no means negligible, as can be seen by the extent to which Hegel himself had to rely on it. Furthermore, Tennemann's discussion of the objects and methods of the history of philosophy is an admirable treatment of a neglected theme. Mr. Walsh does not, however, like Collingwood, say that Hegel's *History of Philosophy* is a *good* history of philosophy, but abstains from judgment.

H. B. ACTON.

Reason and Common Sense. An Inquiry into some Problems of Philosophy. By R. G. MAYOR, with a preface by Sir DESMOND MACCARTHY. London: Routledge & Kegan Paul Ltd., 1952. Pp. xxii + 663. £1 15s.

THE late R. G. Mayor, who died in 1947, wrote this very long book after his retirement from the Civil Service. His undergraduate studies at Cambridge during the late '80s had evidently left him with an intense thirst for philosophy and a deep dissatisfaction with the method of doing philosophy which he experienced there. This book is the record of that dissatisfaction, and of the author's journey through various philosophical positions, from naive realism to phenomenalism, from phenomenalism to idealism, from idealism to solipsism, and from solipsism to a philosophy of the present moment. At each stage the conflict with common sense becomes more acute, yet each stage appears as the inevitable consequence of taking seriously the implications of the preceding stage. Why then should this method of philosophizing lead to conclusions which nobody

believes, and which, if true, imply the non-existence of believers and disbelievers? Mayor's answer is that philosophical scepticism, culminating in scepticism about the reality of sceptics, is the necessary result of neglecting the part played by emotion in human knowledge. Philosophers have too glibly assumed the irrelevance of human feelings to problems of knowledge. In particular, they have neglected the role of self-love, sympathy and a "sense of Nature" in providing each individual with an assurance of his own continuous identity, of the reality of other individuals, and of the existence of the material world. This raises the question, which Mayor faces without fully answering, why these emotional assurances should be considered to possess a superior resistance to the sceptical arguments brought against more conventional accounts based on sense perception. Mayor suggests that one state of mind can be felt to be intrinsically better than another, and that this type of evaluation somehow implies that a better state brings with it a truer perception of reality than would be given by a worse state. These views are linked up with a belief in the objectivity of value-judgments in general, and the conclusion is reached that there is a necessary connexion between ethics and metaphysics, the latter being ultimately dependent on the former. The argument grows dark in these closing stages.

This bare summary suggests the limitations of Mayor's work, but not its virtues. If his discussions sometimes seem remote from present-day methods and interests, it would be parochial to make too much of this. His main achievement is the "remarkable literary skill by which he makes accessible to the general reader complex and subtle philosophical theories", as his editors justly remark. A lucid felicity of expression is sustained in a manner which disarms criticism about the length of the book. Careful, judicious appraisals, of the kind provided here, do not lend themselves to pithy statement. These qualities make the book an admirable guide over ground which every student has to cover. Its price is unfortunate.

R. J. Spilsbury.

Grundlegung und Aufbau der Ethik. By Franz Brentano. Bern: A. Francke, 1952. Pp. 424. No price indicated.

The importance of Brentano's thought and his great influence on recent philosophy makes a complete edition of his philosophical writings highly desirable. Such an edition was started by Oskar Kraus and Alfred Kastil, but their work was interrupted by the advent of national socialism and the second great war and both editors have since died. Franziska Mayer-Hillebrand is now continuing their important editorial work.

The present book consists of lectures given by Brentano in the years 1876-1894 supplemented by Kastil's lecture notes, passages from the other writings of Brentano and, very occasionally, from those of Kraus. These additions are clearly indicated and seem to me helpful and perfectly justified. The main argument is a fuller restatement of Brentano's ethical doctrines as presented in his "Vom Ursprung sittlicher Erkenntnis" and a defence of them against other theories about the nature of moral valuation. The most important new matter is contained in the third part of the book, and deals with the problem of the freedom of the will.

Brentano holds that the concept of moral goodness is an empirical, and not an *a priori* or "non-naturalistic" concept and that, like the concept of truth, it is abstracted from inner experience. Since he believes that the analogy between the notions of truth and goodness is of great importance to ethical theory, his analysis of the former must be indicated, however roughly. 'Truth' is defined in terms of self-evidence which is abstracted from the experience of judging with self-evidence. The judgments of inner perception (*e.g.* the judgment that I recognise myself as seeing something blue) and the apodictic negative judgments (*e.g.* the judgment that the same thing cannot at the same time exist and not exist) are characterised as self-evident. According to Brentano a judgment is true if it is either self-evident or if, although not self-evident, it is in all other respects like some judgment which is self-evident. In other words a true but not self-evident judgment must be logically equivalent to a self-evident judgment.

Just as some of our judgments are characterised by self-evidence so some of our feelings of love and hate are characterised by a kind of self-evidence. One might call the latter "moral self-evidence" in order to distinguish it from the "theoretical" self-evidence which, according to Brentano, is characteristic of some judgments. To state that something is good is to state that the love for it either is characterised by moral self-evidence (*e.g.* our love for knowledge) or else that the love for it differs from a love which has this characteristic only by lacking it. It is, Brentano believes, self-evident that self-evident judgments and self-evident feelings of love cannot be incompatible: and that consequently the notions of truth and goodness are not only empirical but also objective in the sense that their correct application cannot be a matter of taste.

At some points the analogy between true judgments and morally good feelings breaks down. In particular there is in the field of theoretical judgment no comparative truth while within the field of valuation there is comparative goodness. The notion 'morally better' is, according to Brentano, empirical and objective because it is abstracted from feelings of preference which are characterised by self-evidence.

These are the barest outlines of a theory which raises many issues as controversial as they are important. While a critical discussion of them is not possible in a brief notice, it may be said that Brentano's ethical doctrine compares in at least two respects favourably with other types of ethical intuitionism: It does not use the unanalysed notions of non-naturalistic properties and of synthetic *a priori* connexion between non-moral and moral characteristics; and it does not use the legalistic argument that the *onus probandi* lies not with the ethical intuitionist but with his opponent.

Brentano's treatment of the problem of moral responsibility and determinism contains some acute and helpful distinctions between freedom from determining causes, freedom from coercion, freedom of choice and other senses of "freedom". His two main arguments for determinism, however, are not very convincing. They are, very roughly speaking, firstly that determinism is highly improbable and secondly that 'the possibility of chance' is a contradiction in terms.

In conclusion I should like to express the sincere hope that the long delayed complete edition of Brentano's works will soon be available.

S. KÖRNER.

John Stuart Mill's Philosophy of Scientific Method. Edited with an Introduction by ERNEST NAGEL. Hafner Publishing Company, New York, 1950. Pp. l (50) + 461. 30s.

THIS deserves notice because it is so different from those dreary 'selections from the philosophical classics'. The principle of selection here is not to give a digest of Mill's thought, but to re-edit his writings on logic so as to retain Mill's own purpose of providing (i) a 'general theory of proof' in which the analysis of deductive procedures is explicitly subordinated to the theory of induction; (ii) an introduction to the methodology of the social sciences. Professor Nagel has done a wholly admirable job in promoting Mill from 'source' to textbook. The coherence and unity of Mill's *System of Logic* is easily lost on the student of scientific logic, not only because of the pedagogically conventional deductive-inductive dichotomy, but because of the sheer unwieldiness of the book and the intrusion of so many chapters of doubtful importance. By judicious pruning of the eighth edition, Professor Nagel has been able to throw in a handful of chapters from the *Examination of Sir William Hamilton's Philosophy*, as well as the essay *On the Definition of Political Economy: and the Method of Investigation Proper to it*, all within 440 pages of comfortably large print—itself a delight to all those who have struggled with the minute type and double columns of the current (Longmans') issue.

No two people will agree on what to prune. I regret no omissions except chap. ii of Book VI ('Of Liberty and Necessity') which is surely a *locus classicus* for students of the free-will problem?—the excuse that this is ethics, not logic, would be contrary to purpose (ii) above—and I miss, less acutely, Mill's observations on the 'science of history' in chap. xi. And does the principle of selection quite justify the *Hamilton* excerpts on philosophical psychology and phenomenalism, indispensable though they are for Mill's epistemology? The connexion is biographical rather than logical: Mill did indeed confuse the question, how we justify our empirical beliefs, with the question, how we acquire them.

There is an interesting and well-documented introduction, dealing with the social and political background of Mill's logical works, his intellectual development, the sources of the *Logic* and the *Examination*, the purpose of the *Logic* and its main assumptions, the conception of Induction as involving all other logical procedures, and ending with a brief evaluation.

BERNARD MAYO.

Hume's Intentions. By J. A. PASSMORE. Cambridge University Press, 1952. Pp. 159. 18s.

PROFESSOR PASSMORE'S book is solid, sensible, and well written. It is also, though brief, remarkably comprehensive. Hume, like perhaps most other philosophers who have written large books with large intentions, is not often accorded a general survey. More commonly some fragment of his work is picked out and scrutinized by itself, taking on thereby an air of importance disproportionate to its place in Hume's general scheme. This procedure, provided it does not purport to be historical, is not objectionable; there is indeed, according to Professor

Passmore, something entirely "Humean" about it. But it is useful nevertheless to venture from time to time upon broader views; and Professor Passmore is no doubt right in his suggestion that failure to envisage Hume's work as a whole has encouraged some misunderstanding of his intentions, and hence of his place in the history of philosophy. He did not himself intend to fall in as third man in the neat trio Locke—Berkeley—Hume; and in fact this book makes it very clear how curiously little he was influenced by Berkeley's arguments.

As Hume observes, "my design in the present work is sufficiently explained in the introduction". And his design is to "explain the principles of human nature", and thus "in effect propose a compleat system of the sciences, built on a foundation almost entirely new, and the only one upon which they can stand with any security". What he calls "the science of man" is to be laid down as the foundation of all human enquiries; and the method followed is to be "the application of experimental philosophy". In the pursuit of this general aim Hume is only incidentally (and not consistently) a sceptic, a positivist, or a phenomenalist: more fundamentally, but hardly more consistently, he relies on the explanatory power of "association of ideas": but all of these are only planks which he attempts to incorporate in his platform. Professor Passmore reveals with considerable cunning the character of these various planks, but he indicates also the place of each plank in the general structure, and the shortcomings of the design as a whole. It is no doubt a compliment to his skilful discussions that one often wishes they might have been longer.

I think, however, that there are certain points at which lengthier discussion is really required. Hume's bland assumption of the adequacy of his terms "impression" and "idea" is excused (p. 93) on the ground that the view thus assumed was common form at the time; and no doubt this is the case. But the remarkable power of these terms to beg important questions, to generate insoluble problems, and to arise in new dress like the phoenix from their own ashes, would surely justify and even require a more detailed discussion both of their naturalness and their deficiencies than Professor Passmore supplies. It is arguable that Hume's whole programme—his notion that "the science of man" must be the root of all knowledge—derives partly at least from assumptions about the "perceptions of the human mind" which are at once obscure, confused, and question-begging, and which deserve to be closely scrutinized. Secondly, Professor Passmore is perplexingly cryptic in certain remarks about generality. He observes (p. 40) that Hume never considered the possibility that "we are directly acquainted with generality"; he took for granted that everything which exists is particular, in a sense which, according to Professor Passmore, "prevents things from being also universal". He himself seems to suggest that we must deny, that "experience knows nothing of generality", for otherwise (p. 156) we may provoke a rationalist reaction whose leaders would come forward as "the defenders of generality". I do not wish to say that no point of importance is here in question, but I at least find the terms in which the issue is presented entirely incomprehensible. Does experience know something of generality, or does it consist "wholly of what is merely particular"? Are some things universal? I am sure that no answers can reasonably be offered until these riddles are replaced by perspicuous questions.

Thirdly, as a minor question of tactics, I would suggest that the last chapter, "Hume's Achievement", might well have been longer. Each of the earlier chapters culminates in so skilled a revelation of such serious tangles that a final eight pages could hardly suffice to convince the unfriendly that Hume is an important philosopher; and the book as a whole is, I think, intended to rebut the aspersions of hostile critics. Professor Passmore does observe that, with certain rather large adjustments, Hume's views are of great and continuing value. But it might have been prudent to display more fully how this is so; and the author's sound historical sense, his close grasp of Hume's complicated writings, and his talent for exposition, may well make one wish that he had allowed himself a more spacious epilogue.

Justice demands one further comment. Why, if Hume's theory of belief is "an amalgam of psychology and logic" from which "we can cut away the psychology and leave a logic behind" (p. 157), was Professor Price at fault (p. 154) in noting (*Hume's Theory of the External World*, p. 15) Hume's "failure to distinguish philosophical problems from psychological ones"? Professor Passmore rightly stresses the point that this failure was not inadvertent, but on the contrary deliberate, wholehearted, and fundamental. But Professor Price is aware of this, as is shown by his immediately following contrast between Hume's practice and his professions. In spite of his intentions, philosophy keeps creeping in; and no doubt both would agree that it is fortunate for Hume and ourselves that this is so.

On page 154, note 2, the name of Mahaffy is mis-spelled.

G. J. Warnock.

Ethics and the History of Philosophy. Selected Essays. By C. D. Broad. London: Routledge and Kegan Paul, 1952. Pp. xiv + 274. 21s.

Professor Broad has here collected a dozen lectures and papers first given or printed between 1926 and 1949. They are arranged in three sections. The first, largely biographical, comprises essays on Newton, Locke, Sidgwick, McTaggart, and W. E. Johnson. There follow three papers on philosophy of science, covering the philosophy of Bacon, scientific developments and their philosophical repercussions from Bruno to Descartes, and Leibniz's last controversy with the Newtonians. Finally there are four pieces on ethics, including the Cambridge inaugural lecture on "Determinism, Indeterminism and Libertarianism" and a lecture on "Egoism as a Theory of Human Motives" given in 1949.

Of the first two sections little needs to be said. The biographical subjects at the beginning being men after the author's heart, his treatment of them is both lucid and judicious. The papers on McTaggart and Johnson contain useful material for the history of the Cambridge Moral Science School, an important element in the recent history of ideas to which someone might well give attention. In the philosophy of science section the paper entitled "The New Philosophy: Bruno to Descartes" could be read with profit by all elementary students of Descartes's *Discourse*, giving as it does a singularly clear and balanced account of the background to that work. The paper on Leibniz is a critical discussion of the controversy with Clarke about absolute space. But the chief

interest of the book from the philosophical point of view is undoubtedly in the section on ethics.

Broad has long been notable among exponents of logical analysis in refusing to have anything to do with the thesis that philosophical problems should be not solved but dissolved. In his introduction he goes out of his way to reaffirm his belief (though unfortunately without arguing the point) that the question " How, if at all, can induction be justified ? " is a perfectly genuine one. The Cambridge inaugural lecture, in which it was argued that the case for free will is at best highly doubtful, is reprinted as it was given in 1934 ; presumably the author still thinks his approach correct, despite the fact that every sophisticated student of philosophy could now make two or three moves against it. The lecture on psychological egoism, the most recent in the book, takes the case it considers with perfect seriousness, and attempts to answer it by refining on the arguments used by Butler against Hobbes.

Opinions on the value of this procedure may differ sharply. For my part I am glad to see the Morrisonian wing of the Logical Analysis Party showing activity at a time when the Bevanites have a virtual monopoly of public attention, though I could have wished that it had restated its case instead of merely reprinting past pronouncements. This would have been especially valuable over the issue of free will, current treatments of which leave much to be desired. Broad might well claim that some weaknesses of these were exposed by implication in his 1934 lecture, in so far as he argued that words like " ought " and " ought not " can only be known to have a *proper* use if we can justify certain assumptions implicit in their use. To the argument that their use must be accepted as legitimate just because it occurs as part of ordinary language he would reply, I suspect, by denying that we can properly distinguish between ordinary language in the abstract and what the ordinary man who uses it believes. Plain men, if questioned, would probably admit that they do make the assumptions about free action which Broad says are involved in talk about such action ; the question is whether they can discard these assumptions and still go on using the language. What Broad had to say on the subject in 1934 is full of interest, but it would have been still more interesting to have what he had to say on it in 1952.

In his other main ethical essay Broad seems to me less well advised, for surely the thesis of psychological egoism is an *a priori* thesis if there ever was one : there is no sort of evidence its supporters will accept against it. Arguments of the Butler type are hence no more effective against it than were Moore's curiously similar arguments against idealism in 1903.

W. H. Walsh.

John Locke. By D. J. O'Connor. Pelican Books, 1952. Pp. 222. 2s. 6d.

In the conclusion of his book, Mr. O'Connor claims that Locke's Essay is " a philosophical work of the highest order ", in that it " opens up new territories, and provides new techniques for exploring old ones ". This claim is probably not exaggerated. But Mr. O'Connor admits that the Essay contains a great deal of confusion ; and though he leads the reader firmly and doggedly through the text, at the end the truth of his claim for Locke is still obscured by the tangles and inconsistencies of the Essay

itself. Anyone writing a book on Locke would be faced with the choice between going through the text in detail, with all its contradictions, or leaving out a great deal, for the sake of presenting a clear account of the most important points. And perhaps the defect of this book is that it is too thorough; no clear distinction is drawn between those of Locke's questions which have proved fruitful, and those which are trivial, or arise from mere confusions.

For example: in chapter 3 (Ideas and Experience) Mr. O'Connor efficiently sorts out, as far as possible, Locke's distinctions between Simple and Complex Ideas, and Simple and Complex Modes; and at the end of the chapter, at no greater length, discusses Primary and Secondary Qualities, and Locke's views on perception. He mentions in passing that "the rapid growth of physical science" in the seventeenth century gave plausibility to the doctrine of Primary and Secondary Qualities, but he further says that it is difficult to present this kind of view in a form which will stand criticism. Now it is doubtless true that it is impossible to present it so that it will stand thorough criticism, but it seems extraordinarily easy to make it *plausible*. And it is surely a matter of importance and interest that the utterances of scientists have so often tempted other scientists and philosophers (such as Russell) into theories of perception not unlike Locke's. Here is a case, then, where one feels that Locke's theory is worth picking out and discussing, independently of his own complicated vocabulary of Powers and Simple and Complex Ideas. Without such special treatment its interest may be overlooked.

The same criticism could perhaps be brought against the discussion in chapter 5 of Locke's views on personal identity. Although Mr. O'Connor has a section at the end of the chapter entitled "the Unity of the Mind and Personal Identity", it is short compared with the space he devotes in the same chapter to Locke's confusions about reflexion and volition. Here again the reader might have been helped if the more interesting questions had been isolated from the surrounding tangles.

Nevertheless, the thoroughness of the book is in a way its great merit. Nothing is left out. There are, for instance, some sensible remarks on Locke's political philosophy; but every point equally is discussed with brevity, fairness and lucidity; and this is much to be grateful for.

MARY WARNOCK.

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VIII.—NOTES

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